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WITH THE ASSISTANCE OF
C. BURT, J. DREVER, AND S. S. ISAACS

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VOLUME XXI 1930 PART I

PERIODS OF ARRESTED PROGRESS IN THE ACQUISITION OF SKILL.

By M. DRURY SMITH.

(*From the Cambridge Psychological Laboratory.*)

I. *Review of the problem* (pp. 1–3).
II. *Task A: Experimental method* (pp. 3–5).
III. *Periods of arrested progress in task A* (pp. 5–16).
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V. *Task C: Method and results* (pp. 19–22).
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I. REVIEW OF THE PROBLEM.

A STUDY of the literature dealing with the occurrence of plateaus in the process of learning shows a fairly clear-cut distinction between those workers who regard the plateau as due to factors inherent in the learning process, and those who suppose it to be the result of a temporary failure in attention and effort on the part of the subject. The distinction is of practical as well as theoretical importance, because in the one case the plateau would probably be inevitable and essential, while in the other case it might be eliminable by means of a suitable adjustment of incentives.

Those workers who have taken the first point of view have all assumed that the occurrence of plateaus was intimately connected with the fact that the tasks in which they appeared consisted of dissociable parts. Thus Bryan and Harter (1897, 1899) distinguished 'lower-order' and 'higher-order' habits, which in their experiments on the reception of telegraphic messages roughly corresponded to the ability to receive words, and the ability to receive whole phrases and sentences as units. A plateau occurred because the two sets of habits were not improved simultaneously from the start, but there was a period of lag between the perfection of the lower habits and the inception of the higher habits during which no progress was shown. At this time the lower habits were becoming automatic, thus freeing the attention for the higher habits. The standpoint of Swift (1903–1910), in his ball-tossing and other experiments, was essentially the same.

The alternative theory, which ascribes the plateaus to failure of attention and effort, was originally put forward by Johnson (1898), and was elaborated at great length by Book (1908, 1925). According to Johnson, "if the subject could be induced to sustain the same effort day by day, there would be no plateaus." Book concluded from introspections and tests of pulse-rate that less effort was actually put into the work at all those stages of practice where little or no improvement was made. Later, however, he modified this position by stating that the plateaus tended to occur at critical stages in the learning process—at certain definite levels of advancement, and apparently at points where new and increased difficulties were met with.

Bryan and Harter and Swift, in the course of their papers, made statements which to some extent approximated their views to that of Book. Bryan and Harter mentioned that "men whose receiving curve has been on a level for years frequently rise to a far higher rate when forced to do so, in order to secure and hold a position requiring the higher skill." Swift mentioned a lapse of attention as accompanying plateau periods. More recently, Thurstone (1919), in typewriting tests, found plateaus in only 2 out of 83 subjects, and considered that in both cases the explanation probably was that these subjects were offered commercial positions after attaining a certain efficiency, and consequently made little effort to improve beyond this point.

Thus, the difference between the two schools is far less fundamental than it seems at first sight, since on the one hand Book apparently admitted that the plateaus tend to occur at certain definite stages of the learning process, while on the other hand Swift and Bryan and Harter apparently admitted that they may be accompanied by diminished attention and terminated by a renewed effort. The two views are not mutually exclusive.

Batson, in 1916, carried out a series of experiments to test the supposition that plateaus, if they occurred, were probably connected with the degree of complexity of the task as Bryan and Harter had suggested. He confirmed Swift's result that no plateaus occurred in the relatively simple task of ball-tossing. This was still the case even when the subject was directed to concentrate his attention on one or other phase of the game. Batson then devised a more complicated task consisting of four very distinct stages, and from his results concluded that plateaus occurred with those subjects who gave their attention to the separate factors of the task, but not with those subjects who tried to make progress in all the parts simultaneously. The score in which plateaus occurred was the score for successful performance of the whole task. Unfortunately the test was such that successful performance of any one part presupposed success in the preceding parts. Hence, if progress was only being made in the earlier parts, the score as a whole would necessarily show no improvement. The scores and introspections did not conclusively eliminate the possibility that the plateaus were incidental in nature rather than due to a concentration of attention on one part of the task.

Two negative results may, at any rate, be regarded as established by the work done during the last thirty years; first that plateaus do not as a rule occur in the learning of a relatively simple process (Bryan and Harter, 1897, 1899); secondly that even in the learning of a relatively complex process they do not necessarily occur (Batson, 1916).

The experiments which follow were planned in order to gain further evidence as to the existence and causes of plateaus in tasks of three types. Task A was a rather complicated ball-game, 'Ringball,' which was so arranged that fresh elements could be introduced at will, in order to study the process of co-ordinating them with the older ones; or alternatively it could be played at its highest degree of complexity from the outset; or, lastly, the elements could at first be practised separately, one after the other, and later incorporated into a combined movement. Task B was a parlour game known as 'Guidit,' which differed from 'Ringball' in requiring only eye and hand movements; Task C was a simplified type of 'Shorthand,' which differed from the other tasks in involving a larger proportion of mental activities.

II. TASK A: EXPERIMENTAL METHOD.

In bare essentials this game consisted of throwing a rubber ball into a target on the wall, or in causing it to bounce into a target on the floor. These two items could be practised separately or together, and with the right hand only or alternatively with the right and left hands. Two forms of the game were employed.

Ringball, Type 1.

The first section of this game involved only a right-handed throw. The subject aimed at tossing the ball into the right-hand target on the wall, and caught it again after it had bounced once on the floor. To begin with he tried to throw in time to a metronome set at 60 beats per minute, the separate beats marking throw, bounce and catch respectively, so that actually he threw 20 times per minute if the rhythm was perfect. The rate of the metronome was accelerated either when the subject asked for this to be done, or when the speed attained seemed to warrant such a change. The subject was allowed to pass on to the next section when he had scored an average of 16 out of 20, three days in succession.

In the second section this learned movement was combined with others which had not previously been practised. The subject was now required to throw alternately with the right and left hand, and also to endeavour to make the right-hand balls bounce inside the target on the floor. In the third section bouncing into the left-hand target was also introduced.

Each practice consisted of 5 sets, and each set consisted of 20 throws when only one hand was used, and of 40 throws when both hands were used. An interval of three minutes was allowed between each set. One point was allowed for each shot that entered the target on the wall, and 1 point for each time the ball was caught on the return. In the first section of the game these were the only items scored.

In the second section 1 point was allowed in addition for each ball bouncing inside the floor target. Further, when the left hand was introduced 1 point was allowed for each ball thrown into the left-hand target, and 1 point for each left-hand catch.

In the third section 1 point was also scored for each shot bouncing inside the left-hand floor target. Seven subjects practised this form of the game, but two of them not for long enough to learn the first section.

Ringball, Type 2.

A difficulty encountered with the original form of Ringball was that the daily fluctuations were large relatively to the total score. Most subjects started at a score of about 8; while the upper limit was 15 or 16. The daily fluctuations were sometimes as much as 3 or 4 points, so that they masked the form of the curve. The second form of the game was devised in order to provide a larger score, with relatively smaller fluctuations.

The single hit-or-miss target was replaced by a target consisting of three concentric circles, the annular areas between which were painted in different colours.

Ten points were now allowed for each bull on the targets, 6 for the ring immediately outside the bull, and 3 for the outer ring; intermediate scores of 8, 4 and 1 respectively were allowed for shots on the bounding circles. Each practice consisted of 5 sets, each of 20 shots with the right hand and 20 shots with the left hand, the two hands being used alternately.

Of the four subjects who practised this second form of the game, two (B. S. and M. V.) were required to practise the complete movement, both hands throwing and bouncing from the outset. The other two (L. S. and M. M.) began by practising the right and left hands on the same occasion, but in successive sets instead of alternately. After practice 6 they threw alternately with right and left hands, and after 12 practices they attempted also to make the balls bounce inside the floor targets.

In both types of the task it was found that the errors of any one subject usually had a distinct bias to one side or other of the target. In order to obtain a quantitative measure of this bias, the ratio $(R - L)/(R + L)$ was calculated for each set of throws. R represents the number of errors to the right of the target, L the number to the left of the target. The ratio is equal to zero when the errors are equally distributed on either side of the target, and has maximum numerical values of $+1$ or -1 when the errors are all to the right or all to the left. A similar expression, namely $(U - D)/(U + D)$, where U is the number of errors made above the target, and D the number made below the target, serves to define the vertical bias. A decrease in the horizontal or vertical bias did not, of course, necessarily indicate that the errors were less serious, but it did indicate that they were more uniformly distributed about the target, and in certain cases this denoted an improvement in achievement which was not given credit for in the score. Considerations of space prevent the reproduction of any of these figures.

III. PERIODS OF ARRESTED PROGRESS IN TASK A.

The expression 'period of arrested progress,' was used to avoid any preconceived ideas which might be associated with the term 'plateau.' It was arbitrarily defined to include any period during which the score did not improve for six or more successive practices. A few shorter periods were admitted where the arrest was marked and an explanation in terms of day-to-day fluctuations very unlikely.

The nine subjects who learned Task A all showed periods of arrested progress in their total scores, as will be seen from the following table, in which such periods are denoted by the numbers of the practices over which they extended.

Table I. *Periods of arrested progress.*

Ringball, Type 1.

Subject K. P. Practices	Subject D. S. Practices	Subject N. D. Practices	Subject F. B. Practices	Subject R. P. Practices
13–18	8–17	13–29	23–26	14–19
22–28	23–52	(31–49)*	32–35	33–42
29–38	72–79	—	36–44	—
49–53	87–99	—	—	—
55–59	(See Fig. 1)	—	—	—

Ringball, Type 2.

Subject B. S. Practices	Subject M. V. Practices	Subject M. M. Practices	Subject L. S. Practices
22–42	7–17	11–20	6–9
—	(22–40)*	22–26	11–16
—	58–62	30–32	20–27
—	68–89	—	40–44
—	(See Fig. 2)	—	56–64

* Very slight progress was made during this period.

Of the 29 periods thus enumerated, only 12 extended over 12 or more practices, and only 2 over 20 or more practices. The most extreme case was that of Subject D. S., who made no improvement for 30 practices (23–52 inclusive). At the other extremity the list contains one or two periods which might conceivably be merely the result of a previous accidental high score or some such chance effect.

In attempting to account for these periods, evidence was available from two sources, first from an analysis of the curves themselves, and secondly from the introspections. The latter have been used cautiously, and where there was any conflict of evidence the main weight has been placed on the purely objective data given by the curves. In some cases this alone was sufficient to determine the probable cause of the arrest of progress. Only a selection of the learning curves and scores obtained can be given in this paper. These will be found in Figs. 1 and 2 and in Table II.

Fifteen of the periods of arrested progress can be eliminated as being more or less obviously due to accidental factors. The remaining 14 were apparently due to causes inherent in the learning process, for the particular subject.

Table II. *Ringball, Type 2. Total score (thrice-weekly practices).*

No. of practice	Subjects				No. of practice	Subjects		
	B. S.	M. V.	L. S.	M. M.		B. S.	M. V.	L. S.
1	57·1	31·4	45·8	51·3	46	135·4	61·9	98·5
2	82·8	41·2	60·6	68 2	47	131·3	74·7	105·9
3	83·4	46·2	74·2	66·3	48	135·2	77·6	106·3
4	76·8	43·2	66·2	68·1	49	130·3	66·1	112·4
5	82·7	44·6	76·8	69·3	50	137·4	71·9	104·7
6	93·1	56·2	[74·4	72·6	51	137·7	85·6	110·6
7	86·8	[50·4	61·3	80·1	52	134·9	75·4	104·9
8	99·9	52·1	59·6	72·1	53	137·9	84·2	87·0
9	96·0	46·5	67·6]	81·4	54	—	79·2	113·6
10	83·0	42·0	78·5	86·2	55	—	78·8	119·3
11	87·2	48·8	[76 4	[80·7	56	—	87·2	[99·2
12	96·8	53·1	70·3	81·7	57	—	94·1	106·9
13	95·1	51·8	73·9	77·2	58	—	[89·5	105·7
14	93·0	56·6	74·7	83·4	59	—	86·6	*109·9*
15	107·6	48·1	*56·9*	85·4	60	—	78·8	113·4
16	106·4	48·8	62·8]	*77·0*	61	—	93·6	106·4
17	106·7	48·2]	76·7	82·6	62	—	89·9]	105·3
18	116·8	60·2	72·8	82·4	63	—	93·0	95·5
19	117·0	54·8	77·2	77·2	64	—	*76·8*	88·2]
20	122·5	65·4	[64·9	80·8]	65	—	92·2	—
21	122·1	68·2	63·4	88·6	66	—	86·0	—
22	[*99·4*	[*43·6*	65·8	[85·0	67	—	96·7	—
23	113 6	49·2	72·4	84·5	68	—	[80·8	—
24	113·2	45·8	73·2	63·3	69	—	83·7	—
25	120·6	53·4	73·6	85·6	70	—	88·7	—
26	107·6	41·8	76·4	85·2]	71	—	81·2	—
27	107·0	55·6	69·2]	90·4	72	—	80·9	—
28	109·8	52·4	79·0	84·0	73	—	83·5	—
29	123·4	44·2	80·8	94·7	74	—	92·3	—
30	115·2	61·1	94·4	[90·8	75	—	84·3	—
31	121·2	47·4	82·8	88·0	76	—	85·3	—
32	119·8	48·6	94·3	82·4]	77	—	87·4	—
33	119·4	46·6	84·8	99·9	78	—	95·1	—
34	111·0	65·2	88·2	91·7	79	—	87·0	—
35	123·8	64·8	95·4	96·6	80	—	81·7	—
36	119·0	59·6	96·6	92·2	81	—	86·9	—
37	121·2	62·0	94·3	—	82	—	85·8	—
38	116·3	66·8	*86·9*	—	83	—	84·8	—
39	*110·5*	67·0	100·7	—	84	—	91·7	—
40	109·9	64·0]	[87·8	—	85	—	95·1	—
41	121·0	69·8	97·1	—	86	—	96·3	—
42	120·8]	72·4	95·7	—	87	—	93·5	—
43	126·5	65·8	88·2	—	88	—	93·9	—
44	131·4	77·6	85·4]	—	89	—	90·0]	—
45	127 3	*59·3*	104·7	—	90	—	99·6	—

Italic figures denote first practice after a vacational break. [ ] denotes periods of arrested progress.

The periods are classified according to probable causes in the following section.

(*a*) *Causes apparently inherent in the learning process.*

(1) *Concentration on one component of the task.*

The component upon which attention was particularly concentrated sometimes improved, sometimes failed to improve or even deteriorated. In any case the total score did not improve.

(i) *Subject F. B.* (32–35). The subject was mainly preoccupied with learning to hold and to release the ball correctly. "I still have much to learn about holding the ball" (31). "I kept gripping the ball too hard and holding it back" (32). "Quite definitely my main difficulty is to get the ball to leave my hand in the right way" (33). At practice 34 he once got "the proper hold of the ball" and the score began to improve. By practice 35 he had "got into the old swing again, but as soon as I began to think about it the balls went out." After practice 35 there was no further reference in the introspections to the gripping of the ball, except once at practice 40. The score rose steeply at practice 36, although progress was not subsequently continued.

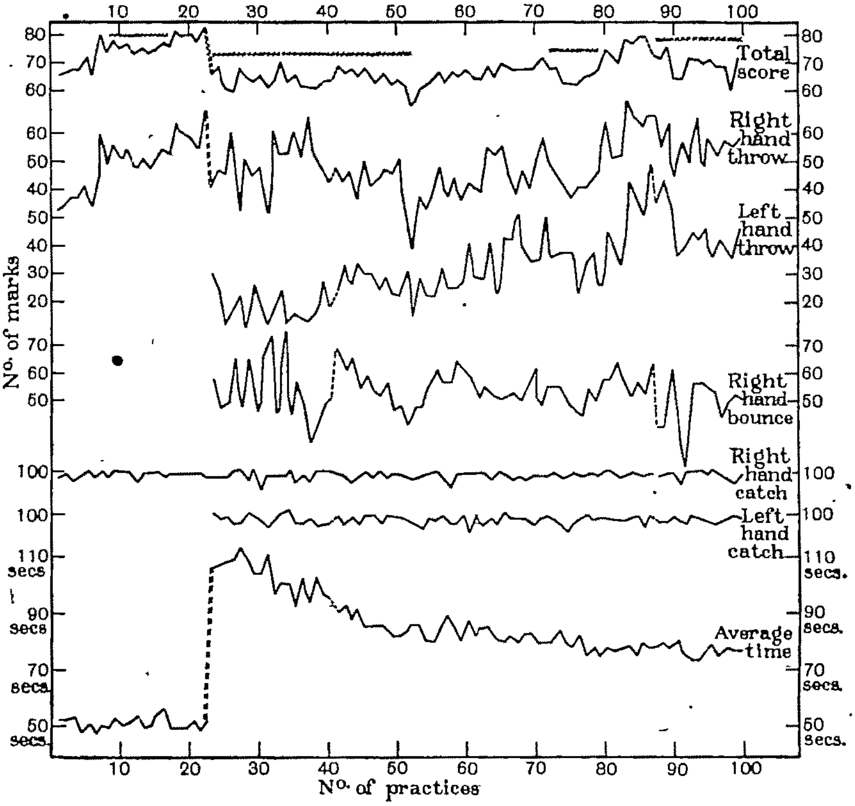

Fig. 1. Ringball, Type I: Curves of subject D. S.

× × × × × × denotes Period of Arrested Progress.
– – – – – – denotes Vacational Break.
= = = = = = denotes End of Section.

(ii) *Subject L. S.* (40–44) (Table II.) The subject resumed after a vacational break at practice 38, and decided that "the weakness of the bouncing was due to throwing the ball too hard: I must try throwing more softly" (38). Improvement followed in the right bounce, but not in the left bounce, and was accompanied by erratic scoring in the right throw. Then at practices 43 and 44 the right bounce fell catastrophically; the subject was feeling off-colour and was still "throwing too strongly" (43). The period terminated with a steep rise and the subject made no further reference to the force of the throwing.

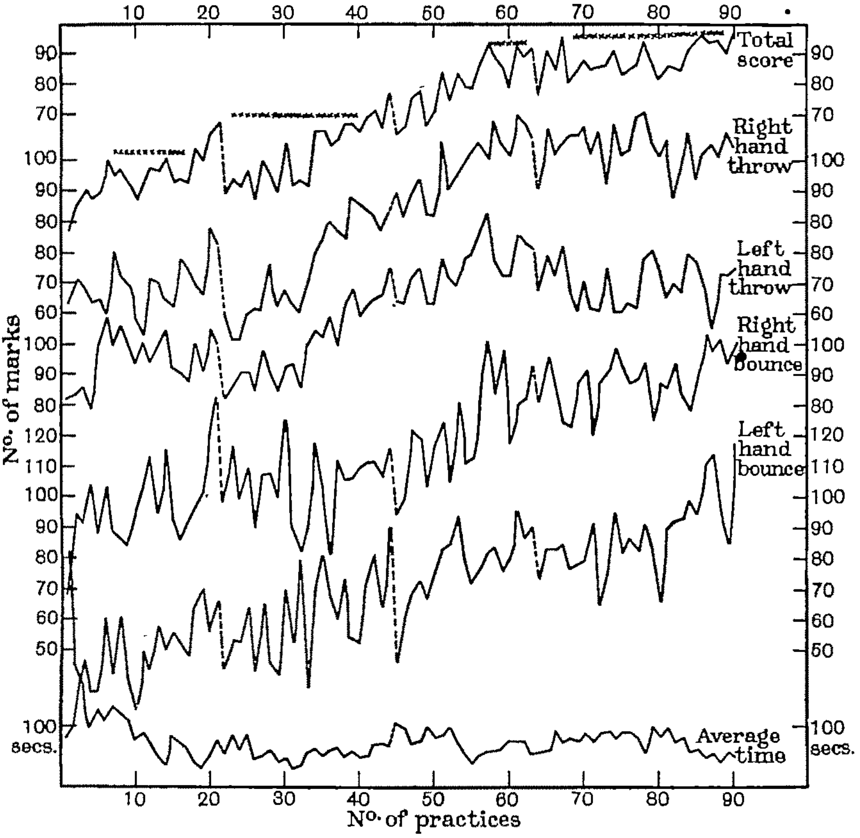

Fig. 2. Ringball, Type 2: Curves of subject M. V.
× × × × × × denotes Period of Arrested Progress.
– – – – – – denotes Vacational Break.

(iii) *Subject M. M.* (22–26) (Table II). The subject was apparently preoccupied with speed, which was greatly improved at practices 22 and 23, and gradually receded from this level in practices 24, 25, and 26. The total score meanwhile was practically stationary except for the very bad practice 24, where the drop was undoubtedly due to the fact that the two practices were done on the same day: "I found myself going faster and faster; aiming at the bulls made me go slower previously" (23).

(iv) *Subject M. M.* (30–32) (Table II). Here the subject was consciously experimenting with different speeds. At practice 30 she "deliberately changed the time and went quite a lot slower, because I was doing it so erratically: I think it would be better if I went slower." At practice 31, "I meant to go slower but forgot, and did better; tried slow movement again, it put me out completely, so I went faster again but remained erratic: I prefer the faster movement, but I feel I might be more accurate if I went slow." At practice 32, "just happened to go faster and found it was better, so I went on going fast." The period terminated in a very marked improvement at practice 33, when the subject "got into a nice swing and felt (she) could have gone on rather well." The speed subsequently decreased, but was not again referred to in the subject's introspections.

(v) *Subject D. S.* (8–17) (Fig. 1). The arrest of progress during this period was attributed by the subject mainly to the fact that she was paying too much conscious attention to the methods of throwing. "I can tell from the movement of the arm whether the shot will go in or not" (9). "I was consciously thinking of the tossing movement of the arm" (11). "I can get neither eye direction nor arm movement" (12). At practice 17, although the score did not improve, the subject felt that she was "getting the movement right but in a disconnected fashion," and sudden improvement followed at practice 18.

(vi) *Subject K. P.* (13–18). This subject made a pronounced improvement in her score at practice 12, and followed with a period of arrested progress lasting for 6 practices, and terminating in another abrupt rise at practice 19. This period was characterized by a preoccupation with speed accompanied by neglect of the other factors. In practice 15 there was a pronounced deterioration in both throwing and catching, although the time remained steady: the subject was "attending to speed all through." At practice 18 attention was divided more evenly between arm movements and speed, and at practice 19, when progress in the score was suddenly resumed, the subject was "not thinking much about the game." Meanwhile, however, there was a falling-off in the speed.

(vii) *Subject K. P.* (29–38). The same subject showed a period of arrested progress between practices 29 and 38. The cause of this was not so clear, since she had previously been recovering from a vacational break, and was at intervals experimenting with new methods of throwing. But the introspections indicated that she was attempting to maintain a particularly high speed of throwing achieved at practice 28, and that it was attention to this factor which arrested progress in the score.

(2) *Oscillation of attention between two or more elements of the task.*

Subject M. V. (7–17) (Fig. 2 and Table II). The subject had made fairly rapid progress until practice 7, but henceforth the total score did not improve again until practice 18. The component scores were very irregular. Attention was directed first to rhythm, "I could not get regular motion" (8); then to the muscular movements, "I sometimes seem to loose control over muscles, just could not get it right" (9); then to bouncing, "I was preoccupied because the balls were not bouncing well" (11); then to the kinaesthetic sensation, "I think I throw much more by the feel of it than by aiming with the eye." Progress was resumed at practice 18, when the subject was "not paying much attention." Practice 20 was "rather good, perhaps because I was thinking of something else and not of what I was doing."

(3) *Co-ordination impeded by conscious attention to it.*

Subject N. D. (13–29). In this one case the most plausible explanation of a period of arrested progress seemed to be that the subject was preoccupied in endeavouring to maintain and make automatic the co-ordination of a particular rhythm with a particular movement of the arm. Though often achieving the one without the other, she only intermittently succeeded in combining them, though a series of particularly good shots was always felt to be the result of such a combination. The subject realized before long that conscious attention probably impeded co-ordination, and in the end, when progress was resumed at practice 30, it was accompanied by the absence of attention and a "funny kind of dazed feeling."

(4) *Interaction between two components.*

This usually occurred at the stage when new and unpractised components were added to the task, to be performed either alternately or successively with old and more practised components. Sometimes the old component was upset by errors transferred from the new and consequently had its progress arrested or turned into a temporary deterioration: sometimes the old movement and rhythm were transferred to the new component and so prevented it from making progress. Although such interaction was noticed by most of the subjects at these stages, it did not always arrest the progress of the total score, since progress was generally made in one part of the tests although it might be held up in another. The following are examples of non-progressive periods in the total score:

(i) *Subject D. S.* (23–52) (Fig. 1). The total score fluctuated about a level which was practically constant for the whole 29 practices, but the component curves—for right throw, left throw and right bounce respectively—showed a well-marked cycle of correlated changes as follows:

23–29: right throw weak and unprogressive.
29–37: improvement in right throw, deterioration in left throw and right bounce.
37–44: deterioration in right throw, improvement in left throw and right bounce.
44–50: improvement in right throw, deterioration in left throw and right bounce.

The relevant introspections, which were numerous and detailed, indicated that at the very outset the left hand (new movement) put out the right hand (old movement); a little later (29 onwards), the right influenced the left; at practice 37 the left hand was tending to upset the right again and continued to do so till practice 44; from practice 45 to 50 the correct movement was being got by the right hand, but this was upsetting the left hand once more. The period ended with a steep fall in all three curves—right throw, left throw and right bounce—this being the first time they had all varied in the same sense. Thereafter the scores for right and left throwing were directly instead of inversely correlated, and slow progress was made in each of them.

(ii) *Subject N. D.* (31–49). This was not strictly speaking a period of arrested progress, since a slight advance was made in the total score, but this was almost negligible in comparison with the big fluctuations of the individual curves, which again, as in example (i), showed an inverse correlation between the right throw on the one hand, and left throw and right bounce on the other, that is to say, between old (practised) and new (unpractised) components.

Between practice 31, when the new components were first introduced, and practice 37, rapid progress was made in speed and right bounce, while the right throw actually deteriorated. The subject felt she was transferring errors made in the new left-handed throw to the old right-handed throw. After a vacational break (between practices 38 and 39), there was deterioration in the new components but not in the old. Between practices 39 and 42 the right throw improved rapidly, the left throw did not improve at all and the right bounce fell away. At practice 43 a synthesis was momentarily achieved but lost again. Between practices 44 and 47 there was rapid progress in the right bounce, accompanied by a deterioration in the right throw.

(iii) *Subject F. B.* (23–26). This subject first practised with the right hand only and then, from practice 19 onwards, with the left hand only. Progress in the latter task was arrested at practice 24. At this stage the subject commented that he "seemed in rather a hurry" (23), and he had previously complained of the difficulty of controlling speed and getting a rhythm. The speed dropped slowly after practice 23. The curves and introspections indicated that the subject began with a speed and rhythm impressed upon him by the previous right-handed practice which was really too fast for the left-handed throwing, at least at this stage. Consequently progress beyond a limited point proved impossible until a new and slower rhythm had been established.

(iv) *Subject R. P.* (14–19). The arrest of progress at this stage seems to have been due to the adverse influence of the new left-hand movement on the old right-hand movement. "The inability to get shots in with the left hand seems to be transferred to the right" (17). "I find I am using the same method of throwing with the right hand as with the left; it is successful with the left hand" (15). As a result, the progress made by the left hand was counterbalanced by the poor scores of the right hand, and the total score remained stationary. Progress was resumed at practice 20, at which stage the subject commented, "I feel little difference between the two hands; both are now part of me; doing it automatically."

(5) *Components possibly not in suitable relative stages for co-ordination.*

Subject B. S. (22–42) (Table II). One very marked period of arrested progress occurring in the curve of Subject B. S. cannot be satisfactorily explained along any of the above lines.

In practices 26, 27 and 28 the subject had done badly in all parts of the task: he was suffering from exhaustion subsequent upon practising for "putting the weight" in University sports. The old score was reattained at practice 29, and then followed the period of arrested progress in the total score. During this time the four component curves fluctuated considerably: on two occasions, practices 30 and 36, there was simultaneous deterioration in all four, while on four occasions there was improvement in three and deterioration in the other one. The relative efficiencies in the four parts of the task also varied considerably from set to set in a single practice. On a balance, the left bounce definitely improved (although it did not exceed or even reach the level of practices 21 and 22), the right bounce definitely deteriorated, the right throw possibly improved somewhat, the left throw definitely did neither. A possible explanation is that the movements could not be co-ordinated until right and left bounces had attained about the same level of efficiency—a result which was eventually secured by simultaneous improvement in the one and deterioration in the other. It is particularly noteworthy that following the vacational break with which this period closed, there was a rapid improvement in the four components of the task. The ground lost during the break was quickly made up, and all four scores rose above the highest level ever previously attained. This fact strengthens the suggestion that the period of arrest was one during which co-ordination in the separate movements was being achieved: the introspections also support this view.

(*b*) *Periods of arrested progress due to incidental causes.*

(1) *Nervous or emotional strain.*

A temporary falling off in the score was sometimes attributable to emotional factors, quite outside the game itself. In addition, there was the particular case of an emotion aroused by the game interfering with the score. This occurred in several instances towards the end of one section of the game, when the subject's score was almost good enough to allow him to qualify for the next succeeding section. Anxiety at this stage led either to nervousness or to excessive caution, with the result that co-ordination was upset and the score deteriorated. Subject F. B., practices 36–44, and Subject R. P., practices 33–42, afford examples of this factor.

(2) *Failure of incentive.*

To some extent failure of incentive operated as a contributory cause in helping to prolong periods of arrested progress originally due to other factors, for the subject often became bored with the game when making no progress, and lost interest or the desire to improve. A falling-off in the score directly due to failure of incentive seems to have been shown by Subject D. S., practices 87–99 (Fig. 1). In this case a new incentive had unintentionally been introduced at practice 83, when the scoring was temporarily taken over by a different person for whom the subject had a great respect, and before whom she was anxious not to appear foolish. The incentive had already begun to wear off before the scorer was withdrawn at practice 90, but nevertheless, after practice 90, the old level was never reattained.

(3) *Attainment of physiological limit and subsequent 'staleness.'*

It is difficult to decide when the physiological limit in a game has been reached, but in one case at least (Subject M. V., practices 68–89, Fig. 2), there was a final period when no appreciable improvement was made for 21 practices. The case is tentatively included under the above heading, but without conclusive evidence.

(4) *Objective physical factors.*

Weather, noise, light, and similar factors were more or less obvious causes of temporarily arrested progress in certain instances.

(5) *Subjective physical factors.*

Similarly, other periods of arrested progress may be ascribed to illness, exhaustion, "incorrect body-set," etc., of the subjects.

(6) *Relearning after a vacational break.*

After a vacational break the subject often, but by no means invariably, resumed with a lower score and only more or less gradually improved. If the previous score be taken as the starting point, there was nominally a period of arrested progress, in the sense defined above, until the point at which the score again exceeded this value. The periods necessary for recovery varied much in duration from a few practices to a whole term (Figs. 1 and 2, and Table II).

(7) *Changes in the method of scoring.*

When one or more new components were added to the game, the score necessarily expressed a different achievement, and consequently the total scores before and after such a change were not strictly comparable. Thus, a temporary arrest of the score when such a modification occurred is not necessarily to be regarded as a period of arrested progress. Examples of such apparent periods of arrest are given by Subject M. M., practices 11–20 (Table II), and by Subject L. S., practices 6–9 (Table II).

(c) *Periods of arrested progress in component scores.*

Most of the component scores, that is to say the separate scores for right throw, right bounce, left throw and left bounce, showed periods of arrested progress; some of which occurred while the total score was still progressing. The following are examples:

Table III. *Ringball: Periods of arrested progress in component scores.*

Subject	Curve	Practices
N. D.	Right throw	31–38
D. S. (See Fig. 1)	Right bounce	59–69
B. S.	Right throw	29–38
R. P.	Left catch	15–24

The majority of these periods of arrest seem to have been caused by diversion of attention or by interaction with some other component in the ways already described.

IV. TASK B: METHOD AND RESULTS.

The task consisted in guiding as accurately and quickly as possible, a small metal ball up to the top of an inclined plane by means of a knitting needle set in a thin wooden handle. On the surface of the plane were 21 holes, each large enough for the ball to fall through, and interspersed among the holes were small wooden barriers which it was necessary to circumvent. After each attempt the subject recorded the time taken and the spot on the board reached. Ten points were scored if the ball got to the top of the plane without mishap, while points were awarded to the other attempts in proportion to the distance up the plane reached. The game was practised by two subjects, K. P. and D. S.

Two separate scores were plotted—the first representing the average mark, *i.e.* the total number of points obtained in a practice divided by the number of attempts; the second the percentage of 'successes.' The average time occupied in the successful attempts was also recorded. The three curves, considered together, showed the varying relationship to one another of the two factors in the game—accuracy and speed.

Table IV. *Guidit. Total scores (thrice-weekly practices).*

	Subject D. S.		Subject K. P.			Subject D. S.		Subject K. P.	
o. of ractice	Average mark	Average time for successes (sec.)	Average mark	Average time for successes (sec.)	No. of practice	Average mark	Average time for successes (sec.)	Average mark	Average time for successes (sec.)
1	1·7	—	1·8	—	29	8·6	8·0	9·4	24·0
2	1·4	—	3·3	—	30	8·6]	7·0	8·8	23·0
3	2·6	20·0	3·6	—	31	*9·3*	*8·3*	9·0	22·0
4	3·5	35·0	4·9	—	32	9·3	8·0	8·3	15·0
5	4·8	26·8	5·6	[39·5	33	9·4	8·0	9·1	19·0
6	4·9	28·2	6·1	37·5	34	9·6	7·5	8·9	19·0
7	5·7	28·9	6·1	35·0	35	9·7	7·8	9·3	19·0
8	6·9	22·9	6·4	29·0	36	[8·9	6·7	[7·7	*20·0*
9	6·9	23·3	6·1	38·0	37	8·7	6·5	7·3	20·0
10	7·3	16·7	6·3	34·0	38	9·4	6·5	7·1	22·0
11	7·7	15·3	6·9	29·0	39	8·8	6·8	7·5	22·0
12	8·0	16·3	7·6	34·0	40	9·3	6·8	7·6	21·0
13	7·4	14·0	7·6	39·0	41	8·9	6·5	8·6	21·0
14	7·2	13·8	7·8	34·0	42	9·2	7·0	7·5	21·0
15	8·2	12·8	7·7	36·0	43	9·4	6·5	8·1	23·0
16	8·1	11·0	7·7	35·0	44	9·1	7·3	8·9	21·0
17	8·5	10·0	7·9	40·0	45	9·3	7·0	8·8	24·0
18	7·8	9·5	8·0	36·0	46	9·1	6·8	8·7	24·0
19	8·3	9·2	8·2	35·0	47	9·4	8·0	9·0	25·0
20	8·7	7·3	8·3	32·0	48	8·9	7·8	9·0	20·0
21	[*5·8*	*8·3*	8·0	38·0	49	9·0	7·3	8·6	22·0
22	7·6	7·5	8·9	38·0	50	9·5	7·8	8·6	20·0
23	6·9	7·5	8·8	37·0]	51	*8·7*	*7·3*	8·9]	24·0
24	6·8	7·3	8·9	22·0	52	9·1	7·0	—	—
25	8·1	7·3	[*8·2*	*23·0*	53	9·5	7·3	—	—
26	8·3	7·8	7·6	26·0	54	8·9	7·0	—	—
27	8·8	7·5	8·4	24·0	55	9·2]	7·3	—	—
28	7·6	7·5	8·8]	25·0					

Italic figures denote first practice after a vacational break.
[ ] denotes period of arrested progress.

Periods of arrested progress.

There were altogether five periods during which progress in either the score or the speed was arrested. These periods occurred as follows:

Table V. *Guidit: Periods of arrested progress.*

Subject	Curve	Practices
K. P.	(i) Time	5–23
(See Fig. 3)	(ii) Marks	25–28
	(iii) Marks	36–51
D. S.	(i) Marks	21–30
	(ii) Marks	36–55

A closer examination showed that three of these, namely K. P., practices 25–28 and 36–51 (Fig. 3), and D. S., practices 21–30 (Table IV), were definitely attributable, in part at least, to breaks in practice and the subsequent necessity for relearning what had been lost. In the former case also, the subject was probably approaching her limit of proficiency. This was still more clearly the case in D. S., practices 36–55, where the

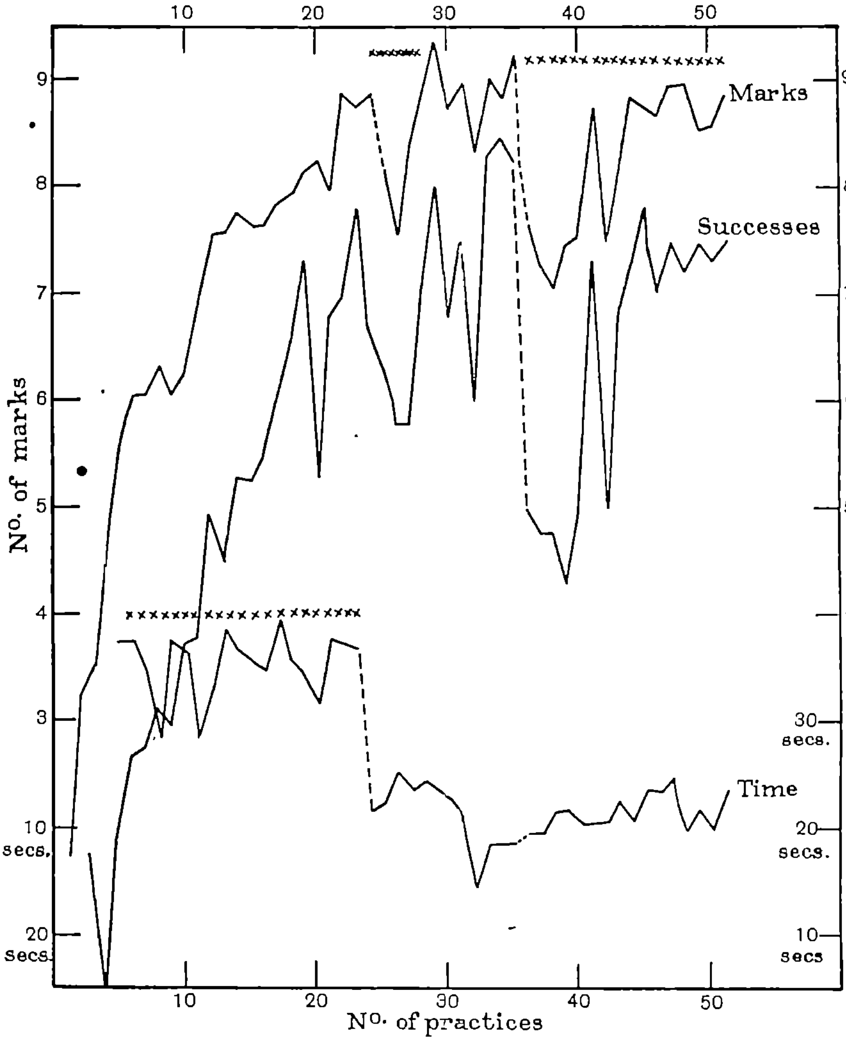

Fig. 3. Guidit: Curves of subject K. P.

× × × × × × denotes Period of Arrested Progress.
– – – – – – denotes Vacational Break.

mark-curve had become level at about 92 per cent. and the success-curve at about 85 per cent. of the maximum theoretically possible.

There remained only one other example of a period of arrested progress, namely that in the time-curve of K. P., practices 5–23 (Fig. 3). The evidence is that progress in speed was arrested at this period because the subject was concentrating on accuracy. As regards a causal classification the period falls under a heading already discussed (p. 7) in connection with Task A, namely arrest due to concentration on one component of the task. This component improved, but the other component, at first, deteriorated.

V. TASK C: METHOD AND RESULTS.

This task involved mental as well as muscular learning, and was studied in order to compare the results with those obtained from the more purely muscular games of skill. The subjects were required to practise a phonetic system of shorthand. They were instructed in the subject-matter during the lessons which preceded the first seven practices. The actual practices consisted of three tasks:

1. *Copy test.* The subject was supplied with a reading book containing words of only one syllable, and was required to transcribe into shorthand as much as possible in five minutes.

2. *Dictation test.* This was introduced at practice 8. The subject was required to take down in shorthand a section from one of the 'fourth leaders' of *The Times*, which was dictated to him by the experimenter for five minutes, at the rate of one syllable per second, the rate determined by a muffled metronome.

3. *Speed test.* A given sentence had to be repeated in shorthand as many times as possible in five minutes. This was introduced at practice 4, and was continued until the limit of improvement had been reached.

In the copy and dictation tests one point was allowed for every word correctly written. In the speed test one point was given for each correct copy of the sentence.

The periods of arrested progress in the curves of the three subjects may be summed up as follows:

(i) *Arrested progress due to incidental factors.*

In all, 24 such periods occurred—9 in the case of K. K., 6 in the case of D. B. and 9 in the case of K. P. They were explicable as the outcome of such factors as the physical or mental state of the subject, distracting noises, relearning after vacational breaks, etc.

Table VI. *Shorthand. Total scores (thrice-weekly practices).*

No. of practice	Subject D. B.		Subject K. K.		Subject K. P.	
	Copy	Dictation	Copy	Dictation	Copy	Dictation
1	2	—	12	—	9·5	—
2	4	—	16	—	11	—
3	8	—	23·5	—	21	—
4	10	—	39	—	13	—
5	5·5	—	47	—	16	—
6	18	—	53·5	—	32	—
7	26	—	51·5	—	25	—
8	31	37	70	48·5	27	41
9	29	36	89	51	49	55
10	35	45	83	51·5	46·5	61·5
11	36	29·5	98	85	54·5	61
12	44·5	37·5	115·5	77·5	54	64·5
13	45·5	49·5	127·5	93	67	100
14	56	62	115	100	68·5	83·5
15	62·5	61	142·5	127	78·5	89·5
16	59	48	136	98·5	84·5	95
17	*47*	*31·5*	151	118·5	76	98·5
18	59	48	174·5	113	*68·5**	*83·5**
19	63	55	163·5	126	67·5	94·5
20	44	44·5	169	105·5	65	107
21	79·5	49·5	*125·5*	*107*	65	[99
22	78	59	116	117·5	80·5	107·5
23	92	73	152	124	73	106
24	94·5	72	146	126	92	106
25	89·5	93·5	159	116	98·5	107
26	118	[73·5	200	152·5	93·5	98·5]
27	123·5	89·5	196·5	150·5	99·5	112
28	138·5	86·5	207	144	89·5	117·5
29	[116	93	212·5	153	108·5	128·5
30	106·5	79	217·5	154	116	122·5
31	128	82	243	159	117	116·5
32	106·5	75·5	224	174·5	115	134
33	120	74]	*209*	*160*	127	144
34	109	97·5	216·5	159	118·5	137·5
35	119·5	86	251	166	133	151·5
36	99·5	104·5	252·5	186·5	142	[144·5
37	130]	106.5	264·5	200·5	131	136
38	141·5	[97·5	230	190	117·5	143·5
39	135·5	86·5	259·5	176	135·5	147·5
40	159·5	99	272·5	177·5	136	151
41	138	83]	276·5	180·5	156·5	146
42	150·5	109	269·5	180·5	136·5	146
43	146	97	*238·5*	*163*	159	140·5
44	159·5	132	246·5	166	159	144
45	154·5	104	269·5	197·5	163·5	145
46	162·5	99·5	281	199	126	139
47	166	96	259	219	160	151
48	183	90·5	303·5	183·5	156·5	145
49	[*130·5*	*83*	280·5	191	145	141]
50	126	99·5	260	195	156·5	157
51	155·5	122·5	296	211	163·5	149
52	163	[105·5	318·5	210	171	167
53	162·5	106·5	272·5	225·5	178	160
54	194	104·5	275	209·5	170	161
55	154·5	114	—	—	153	169
56	157	105·5	—	—	168	141
57	162·5	116	—	—	162	145
58	168	124	—	—	166	161
59	149	120	—	—	161	162
60	172·5	116	—	—	177	145
61	159	105	—	—	170	159
62	151	111]	—	—	159	148
63	176	133·5	—	—	169	158
64	176]	140	—	—	178	155
65	189	145	—	—	187	171
66	*133·5*	*124·5*	—	—	190	174
67	185·5	141·5			169	169
68	172	142·5			171	190
69	143	146·5			189	[187
70	174	147			178	181
71	163	142			203	177
72	189	149			[186	151
73	197	173			178	156
74	214·5	163			203	173
75	185	136·5			184	181
76	189	151			198	173
77	214·5	171·5			176	175
78	214·5	167			172	166
79	214·5	198			192]	184
80	204	163			213	170
81	222	190·5			210	167
82	219·5	189			205	156]
83	228	154			216	196
84	—	—			216	182
85	—	—			218	182
86	—	—			221	200
87	—	—			229	192
88	—	—			*157*	*157*
89	—	—			183	153
90	—	—			180	190
91	—	—			203	195
92	—	—			218	175
93	—	—			204	173
94	—	—			195	177
95	—	—			231	184
96	—	—			210	177
97	—	—			249	190
98	—	—			217	172
99	—	—			200	195
100	—	—			232	174
101	—	—			250	204
102	—	—			230	193
103	—	—			214	194
104	—	—			214	190
105	—	—			241	194
106	—	—			248	177
107	—	—			230	188
108	—	—			288	203
109	—	—			257	[196
110	—	—			268	182
111	—	—			[235	198
112	—	—			264	197
113	—	—			260	181
114	—	—			260	195]
115	—	—			262	208
116	—	—			246	206
117	—	—			241	192
118	—	—			256	207
119	—	—			256	212
120	—	—			264	188
121	—	—			244	201
122	—	—			260	206
123	—	—			258	200
124	—	—			232]	216
125	—	—			276	216
126	—	—			276	218
127	—	—			253	217
128	—	—			269	200
129	—	—			279	198
130	—	—			293	204
131	—	—			292	210

* From then onwards practised daily (except Sundays).
Italic figures denote first practice after a vacational break.
[ ] denotes period of arrested progress (inherent).

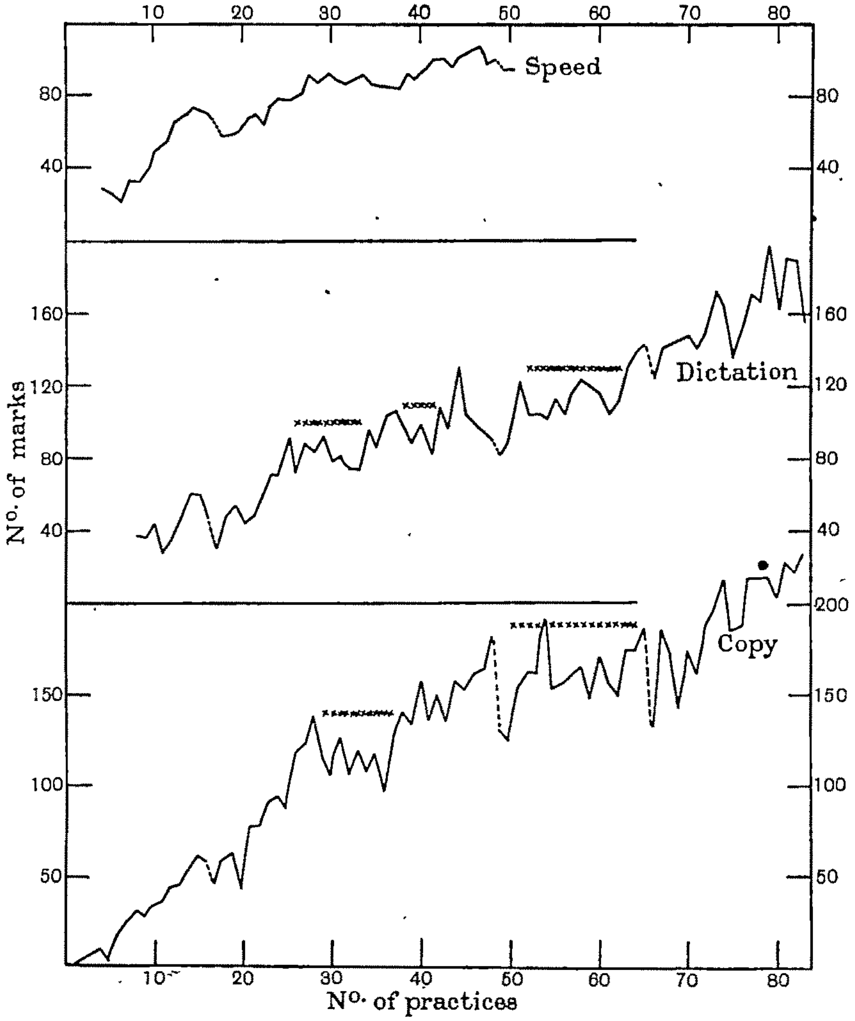

Fig. 4. Shorthand: Curves of subject D. B.
× × × × × × denotes Period of Arrested Progress (inherent).
– – – – – – denotes Vacational Break.

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(ii) *Arrested progress due to factors inherent in the learning process.*

Eleven of such periods occurred—6 in the case of K. P., 5 in the case of D. B. They are shown in Tables VI and VII.

There was no obvious explanation for the arrests in K. P.'s copy-curve. The arrests in the dictation curve were probably caused by the difficulties successively met with in compounding words of two, three, or more syllables. The arrested progress in D. B.'s dictation curve, practices 26–33 (Fig. 4), also seemed due to the difficulty of compounding long words. The progress of D. B. was arrested simultaneously in both copy and dictation tests from about practices 29–40 and again from about practices 50–62. In each case the apparent cause was a temporary inability further to increase the speed of working, when accuracy at that stage had been almost perfected. This inability was probably due to the subject's initial determination to be accurate at all costs, however slowly he went.

Table VII. *Shorthand: Periods of arrested progress* (*inherent*).

Subject	Curve	Practices
K. P.	Copy	72–79
"	"	111–124
"	Dictation	21–26
"	"	36–49
"	"	69–82
"	"	109–114
D. B.	Copy	29–37
"	"	49–64
"	Dictation	26–33
"	"	38–41
"	"	52–62

(See Fig. 4)

Gilbreth (1918) showed that the muscular activities of a fast movement differed from those of the same movement done slowly, and here the original neglect of the speed factor apparently resulted in an incapacity to quicken the rate automatically when it became desirable to do so.

VI. General discussion of results.

A very obvious feature of most of the learning curves obtained in these studies was the large day-to-day irregularities which they manifested. The irregularity was much greater in Ringball than in Guidit or Shorthand, indicating perhaps that the building up of a complex system of co-ordinated body movements was, for most subjects, a more uncertain and haphazard business than the acquirement of refined hand and eye co-ordination (Guidit), or of co-ordination on fairly familiar lines of mental and physical elements such as those involved in Shorthand.

The effect of day-to-day fluctuations was sometimes to conceal, sometimes to exaggerate, those periods of arrest or of actual deterioration which undoubtedly did occur in the large majority of the learning curves. It was not very difficult, however, to pick out non-progressive periods of considerable duration. Any series of six or more practices, during which the initial score was not again reached or exceeded, has been considered a period of arrested progress demanding explanation. In accordance with this definition, some 71 periods of arrested progress were found in the records of the total scores of the 14 subjects. Of these, 43 can be more or less definitely written off as due to incidental causes, objective or subjective. Twenty-eight have been concluded to be due to factors inherent in the learning process.

In the first place it was clear that these periods often occurred when the subject was paying particular attention to one component in a complex task. This has been the case with all three tasks studied, but with an important difference. In the simpler muscular game, Guidit, and in the predominantly mental task, Shorthand, attention to one component of the task usually resulted in improvement being made in that component, although more often this was neutralized by the deterioration occurring in the other components from which attention had been diverted. In Ringball, however, although the above result sometimes followed, the characteristic effect of a concentration of attention on one part of the game was that the factor concentrated upon, equally with the others, failed to improve. Attention in this game seemed, in the majority of cases, to be definitely harmful both to the factor particularly attended to and to the score in general. The various subjects frequently stated in introspections that at the times when their best scores were made they were not paying conscious attention to any part of the game, but were "in a sort of trance," "thinking of other things," "experiencing a dizzy kind of feeling," etc. This is not to say that attention to the game as a whole, or even to the individual parts, was not in the long run necessary to improvement: on this point the data yielded no critical conclusions one way or the other. In a game such as Ringball, where proficiency obviously depends upon a great number of delicate and intricate muscular adjustments, most of them definitely outside the normal range of the subject's awareness, it may be that conscious attention to one so-called component represents only a crude interference with the co-ordinated complex, and one more likely to have harmful results than otherwise.

Two special cases of the arrest of progress in Ringball due to conscious attention may be recalled—one in which there was an oscillation of attention backwards and forwards from one component to another (p. 11), and one in which improvement seemed to be impeded by conscious attention to the process of co-ordinating movements (p. 11).

A further important group of cases occurring only in Ringball may be regarded as effects of involuntary interaction between one part of the task and the other. Presumably such interaction only occurs in a task which includes two or more parts similar in character without being identical—for example, when a given movement is made first with the right hand, then with the left. In Ringball, Type 1 it was customary for the right-hand throw to be practised and brought to a certain pitch of improvement before the other components were introduced. The result was often seen in an inverse correlation between the scores for the right throw and the left throw, and also for the right throw and right bounce. Moreover, the errors in the new movements tended to be carried over and repeated in the old movements, for example, from the left throw to the right throw. A contrary effect also occurred in some cases—the rhythm and habits of the old movements being involuntarily carried over into the new movement, for which they were not in the majority of cases suitable. For example, when a certain speed of throwing had become habitual to the right hand, the same speed tended to be adopted with the left-hand throwing when this was introduced, although it was evident that this speed was too fast for accuracy in the less practised movement.

In a wide sense all these suggested causes of arrested progress may be summed up in the phrase 'difficulties of co-ordination.' Co-ordination may apparently be impeded by undue attention to one component of the task, or by an oscillation of attention from one to the other, or even by the conscious effort to co-ordinate. Apart from attention, correct co-ordination may be delayed by interactions between the components, for example by the carrying over of errors and habits from one movement to another. And lastly, there is some reason for thinking that co-ordination may be checked when there is too great a disparity between the proficiencies in the separate components, and that in general successful co-ordination may require a certain relation between the skills reached in separate parts of a complex task.

If arrested progress is due in general to difficulties of co-ordination, it would be expected to occur only in a complex task, as was in fact the case in Batson's experiment. But in Ringball, Type I there were occasionally periods of arrested progress in the first section, confined to right-handed throwing, as well as in succeeding sections of the game. It must be remembered that the distinction between simple and complex tasks in this connection is subjective rather than objective. The real difference is between the task regarded by the subject as a unit and the task regarded by the subject as consisting of more than one component. Even the simple right-handed throwing of the first section of Ringball, Type 1 fell into the second of these categories in the case of some subjects, for they distinguished between speed and accuracy as things to be separately striven for, although speed was not included in the total score, and they consciously separated such factors as 'holding the ball,' 'force of throwing,' and so on.

The three tasks differed very considerably in the facility with which they could be split up in the subject's mind into separate components. In Ringball the separation of the task into parts was almost inevitable at first, even when, as in Type 2, the subject began his practice with the full task, and was quite inevitable when, as in Type 1, it was practised section by section. On the contrary, in Guidit and Shorthand there were no such very obvious components, although in most subjects' minds speed and accuracy were distinguished as factors which could be attended to separately, at least in the early stages. The occurrence of the periods of arrested progress reflects the difference between the tasks, for while in Ringball every subject showed one or more such periods, in Guidit one of the two subjects, and in Shorthand one of the three subjects, showed no such period, apart from those due to accidental causes. The general conclusion can be drawn with some confidence that in a task which the subject regards as being made up of distinct components or phases, there will generally be periods of arrested progress, but in a task which the subject regards as a unit, such periods will probably not occur. The arrest in progress in the first case is due to difficulties of co-ordination such as have been discussed above.

Two instances of arrested progress, both occurring with Subject K. P. in Shorthand, have not received any explanation in the text. In Shorthand there was much less possibility of analysing the score into its components, and maybe if this could be done these two periods would be found to fall into one of the classes of causes already discussed.

One other period, which must be separately mentioned, occurred in Ringball, Type 2, Subject B. S., practices 22–42 (Table II), where progress was not achieved until approximately the stage at which an initially less skilled movement had become equalized with a similar but more skilled movement (p. 13).

This suggests an interesting line for further research. Can movements separately but simultaneously learned and improving at different rates be co-ordinated at any stage, or only at one or a few stages where there is a definite ratio of skill between them? May co-ordination be impossible at any stage, except at the very outset, if the rates of improvement are widely different ?

The periods of arrested progress did not appear to be characteristic of any particular stage in the acquisition of skill. In one instance such a period began as early as the eighth practice, and in general they appeared to be scattered promiscuously among the curves. Most of them were brief—extending over fewer than 10 practices, but periods of arrest for 10 practices or more occurred on twelve occasions in Ringball, and on a few occasions in the other games. Moreover, they did not seem to fall into two classes such as have been distinguished by Swift, Book and others, under the headings of 'short periods of arrest,' and 'true plateaus.' The duration of the periods of arrest in these experiments was anything from the lower limit fixed by definition, *i.e.* six practices, to an upper limit of 30 practices in the most prolonged case.

The form of the score curve during these periods was also not a constant and characteristic one. Sometimes it was approximately horizontal, but more often than not it showed alternating deterioration and recovery, and in a few cases almost steady deterioration. Again, even when the curve of the total score was approximately horizontal, further analysis showed that the component scores, when as in Ringball, the score was analysable, were rarely or never horizontal also. The constancy of the total score was then merely the result of the fact that the fluctuations of the component scores happened to cancel each other out. It is clear that if they cancel each other out with one method of scoring, in which they are weighted relatively to each other in a certain way, they would not do so under another method of scoring which weighted them differently. And since the weighting is more or less arbitrary, the constancy of the total score curve, when it occurs, appears to be doubly accidental.

The period of arrested progress usually terminated in an abrupt but often only temporary resumption of progress. Only in a few instances were periods of arrested progress followed by prolonged periods of accelerated progress. It is noteworthy that the longest of all the periods of arrested progress, that of Subject D. S., practices 23–52 (Fig. 1), was not followed by prolonged or greatly accelerated progress.

The plateau as a definite single phenomenon with a single cause seems to become more elusive the further it is pursued. What actually appears to occur, in the tasks here studied, is rather a whole series of arrests of progress, some brief, some prolonged, some due to outside causes, and some to causes inherent in the learning process, some eventuating in accelerated progress and some in a mere resumption of the old rate.

The writer's thanks are due to Mr F. C. Bartlett for much helpful advice in the planning and carrying out of these experiments.

VII. SUMMARY.

The curves of learning in three selected tasks were found to show, in addition to the daily fluctuations of score, various periods of greater or shorter length during which progress, as measured by the score, was completely arrested.

About 60 per cent. of these periods could plausibly be accounted for by accidental factors, the remainder were apparently inherent in the learning process itself.

Analysis of the score curves and a study of introspections indicates that progress was arrested in one or other of two sets of circumstances:

(*a*) When the subject's attention was consciously centred upon one component in the task. Sometimes in this case the factor upon which attention was concentrated failed to improve, at other times this factor improved, other factors simultaneously deteriorating.

(*b*) When there was an involuntary interaction between two or more components, so that habits were carried over from one to the other. This usually occurred when new components were introduced side by side with older and more practised components. Sometimes the errors of the new movement were carried over and repeated in the old movement, at other times the rhythm and habits of the old movement were carried over into the new movement; to which they were not at this stage suited.

In a wide sense all these causes of plateaus may be summed up in the phrase 'difficulties of co-ordination'—a successful co-ordination seeming to involve a certain distribution of attention and a certain ratio of skill between the individual components.

Periods of arrested progress tended to occur when the subject regarded the task as made up of separable components, but not when the task was regarded 'as a whole.'

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PSYCHOLOGICAL TESTS AND ACCIDENT PRONENESS.

By C. S. SLOCOMBE and E. E. BRAKEMAN.
(*Personnel Research Federation, New York, N.Y.*)

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VI. *Relationship found* (pp. 35–37).
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I. Introduction.

A great amount of psychological data that has been gathered in the past 20 or 30 years has had little application outside university laboratories. This application has been almost solely in the field of education, but there is a growing recognition of the need of psychological methods in industry and an increasing number of practical applications. The great need for consideration by trained psychologists of the personality and ability of individual men has been almost entirely lost sight of in large companies. The present study was undertaken after a careful survey of all relevant work upon which there was available information (1), and the reported investigation built on such work as was applicable.

The work was undertaken for the Boston Elevated Railway, in which 2300 men are employed in the actual operation of surface cars. Although their accidents compared very favourably with other American street railway companies, the management felt that something more could be done to control the accidents, and caused this investigation to be undertaken in 1927. The definite aim was reduction of accidents and, particularly, reduction in collision accidents.

Analysis soon made it clear that a considerable proportion (55 per cent.) of accidents were caused by a relatively small percentage (27) of the men. It was decided that a large reduction in accidents could be effected if the accident frequency of these men was brought down to the level of the remainder of the men.

As the main subject of the research, 472 men (called for convenience 'high accident' men) were selected and specially studied. Each man was thoroughly studied; his accidents, their nature, etc., his health, medical condition, personality, operating habits, etc. He was then treated in such manner as seemed desirable. Diagnosis in most cases was clear, but in some cases it was very difficult to determine the reason for a man's accidents. It was suspected there might be difficulties in reactions which psychological tests would reveal.

II. TESTING LABORATORY.

Tests were therefore designed and a testing laboratory set up. The types of tests used were determined partly by type of accidents and partly by necessity of testing factors as general as possible, that is to say, not specific to the laboratory. For the purpose of trying out the tests, it was originally intended to take 100 men, 50 high accident men and 50 low accident men. These numbers, owing to absences and so forth, were reduced to 43 of each class. The men tested were motormen varying in age from 27 to 72 years. They were taken from one division so that conditions of discipline and operation could be as similar as possible. In the testing of the men the employment manager, not a trained psychologist, gave the men their instructions under the supervision of the experimenter. This was for the purpose of making the men unstrained in their attitude.

While it was desirable to make the tests appear as practical as possible to the men, it was considered that the actual use of street car machinery in the tests was unsuitable because of the gross muscular movements involved, which were likely to obscure the functions to be tested. Nevertheless, it was not impossible to have the tests appear practical to a certain extent and obtain the interest of the testees. The tests were mainly of simple psychological functions rather than of complex practical situations.

III. DESCRIPTION OF TESTS.

These were the factors it was decided to measure: perseveration, oscillation, speed, accuracy and muscular control.

Perseveration was measured in four tests.

(1) Choice reaction test. In the first test the subject was instructed to respond to a red light by pressing a button with his right

hand, and to a green light by pressing a button with his left hand. The test was then repeated, the subject responding to the red light with his left hand, and to the green light with his right. The differences between the number of omissions and mistakes and the speed of reaction in the first and second tests was used as two measures of perseveration. If these differences were zero, or showed that the second test was performed more quickly or accurately than the first, the subject was considered a non-perseverator.

(2) The tapping test as used by Lankes was adopted (2). Two 15-second periods of tapping were counted, and the average taken as the perseveration score. The test, possibly owing to the misunderstanding of instructions to the motormen, proved unsatisfactory as will be shown in the correlation table.

Two other tests were given, but could not be included in the final results owing to mechanical difficulties in the apparatus. The flicker wheel was used (2). Discs of red and green, yellow and blue, orange and slate, and black and white, were used with the various improvements of Lankes. An electric motor was used rather than turning the wheel by hand in an effort to obtain uniform results. Correlation of the test was zero and, owing to mechanical difficulties and misconceptions on the part of the men which were hard to overcome, it proved itself unsatisfactory.

On the dummy control the subject was instructed to swing the control handle around in 9 seconds, pausing a second in each of the eight notches. Then the resistance was raised on the shaft of the handle, making the handle harder to turn, and the test repeated. This was carried out at a still greater resistance and, finally, at the first resistance. The subject was given two trials at each level of resistance. A comparison of the averages of the scores on the two trials at each degree of resistance was intended as a measure of perseveration. As this test was not given to all the men, these scores could not be included in the final results.

Oscillation was measured by variations of the speed in two tests and accuracy in the third.

(1) Oscillation in speed of simple reaction. The subject reacted to an irregular flashing of a green light by pressing a button with the forefinger of each hand. The degree of variation in speed of reaction in the test with the right hand was the score used.

(2) Oscillation in speed of choice reaction. The test as it was given for perseveration the first time was scored for variation in speed of reaction time.

(3) Oscillation in accuracy, the six-light test. There were six lights which flashed one at a time in varied sequence on the table in front of the subject, to which he responded by pressing the button corresponding to the flashing light. The period of testing was divided into six parts, and the variation in errors in these parts taken as the measure of oscillation.

Speed of reaction time was measured in four tests, three being reactions to light and one to sound.

(1) Speed of simple reaction to light. The simple hand reaction to light test as used for oscillation was scored also for reaction time. A complete test was made on both the right and left hands, and the medians of the two averaged and taken as the speed of simple hand reaction to light.

(2) Speed of choice reaction to light. The choice reaction test as described above was used. The median length of reaction time was taken as this measure.

(3) Speed of complex reaction to light. A dummy street-car controller was set up in the laboratory on a platform. On the wall in front of the subject standing at the controller were four lights, red, yellow, green and blue. The subject stood on the platform facing the wall, with one hand on the brake handle and one hand on the control handle. As the lights flashed the man was instructed to respond to them in a definite manner. The instructions followed the habitual reactions to subway lights. Owing to the mechanical clumsiness and lag in recording, the reaction times were so inaccurate as to be of little use and no record was preserved. It was, however, decided to keep this test and give it as an introductory one, since the men were highly interested in it and it formed the link between their street car operation and the laboratory tests.

(4) Speed of simple reaction to sound. In this test a series of noises including the sounding of a street car gong were given as stimuli, and the subject required to select and react to the noise of the street car gong only. It might well be called a sound discrimination test. It was performed with both right and left feet. Correlations of the reaction times under these conditions with speed of simple reaction to light stimuli was high (0·87), and the results were not used in correlation.

Accuracy was obtained in three scores.

(1) Number of omissions in the simple hand reaction to light test.

(2) Number of mistakes and omissions in choice reaction to light.

(3) Number of omissions and mistakes in the six-light test.

To measure *muscular control* the subject swung the controller handle through the eight notches in 9 seconds, taking a second to each notch. It was proposed to use the time interval between each notch in two trials as a measure of steadiness and ability to complete the task in 9 seconds.

In all these tests, with the exception of tapping and the flicker wheel, the stimuli were automatically set off and the responses automatically recorded (3).

IV. RELIABILITY RESULTS.

The first step was to discover whether the scores of the test could be regarded as reliable; *i.e.* did they measure the functions they were intended to measure? In view of the expense and difficulty of repeating the tests on the same men, the criterion of reliability was taken as inter-correlation of the tests. It is obvious that tests which are in themselves unreliable cannot possibly correlate with other tests. This inter-correlation is shown in Table I.

Table I. *Inter-correlation of tests.*

($N = 86.$)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) Speed Simple reaction	—	·45	·48	·39	·37	·75	·34	·05	·06	·03	·02
(2) Speed Choice reaction	·45	—	·32	·40	·31	·46	·44	·16	–·05	·04	·002
(3) Accuracy Simple reaction	·48	·32	—	·43	·47	·44	·23	·02	–·05	– 07	–·001
(4) Accuracy Choice reaction	·39	·40	·43	—	·43	·37	·41	01	–·03	– 26	·24
(5) Accuracy Six-light test	·37	·31	·47	·43	—	·37	·32	·17	–·03	–·06	·21
(6) Oscillation—Speed Simple reaction	·75	·46	·44	·37	·37	—	·53	·06	·12	·05	·01
(7) Oscillation—Speed Choice reaction	·34	·44	·23	·41	·32	·53	—	·13	–·07	·02	·07
(8) Oscillation—Accuracy Six-light test	·05	·16	·02	·01	·17	06	·13	—	·06	–·16	·07
(9) Perseveration—Speed Choice reaction	·06	–·05	–·05	–·03	–·03	·12	–·07	·06	—	·29	·11
(10) Perseveration—Accuracy Choice reaction	·03	·04	–·07	–·26	–·06	·05	·02	–·16	·29	—	·001
11) Tapping	·02	·002	–·001	·24	·21	·01	·07	·07	·11	·001	—

The following points on this table are to be noted:

(1) Four tests, oscillation in six-light test, perseveration as measured by speed and accuracy and by the tapping test were so low

in their inter-correlations (average 0·04) that they were regarded as so unreliable as to be disregarded.

(2) It was originally intended to discover whether oscillation and perseveration showed up as group factors. Owing to the fact that all the perseveration tests proved unsatisfactory and one oscillation test, it is impossible from the remaining significant correlations to determine anything about the presence of these group factors.

(3) The remaining seven tests show an average inter-correlation of 0·42.

(4) Application of the tetrad differences formula gives a maximum tetrad difference of 0·10 with probable error of 0·03.

(5) This shows the rather surprising result that among these different tests of reaction there is nothing but a general factor and specific factors. The nature of this general factor is, of course, entirely unknown. It may possibly be called 'reactibility' or some such function. It may or may not be related to intelligence. It is hardly likely to be solely intelligence.

(6) Correlations of each test with general factor are shown thus (4):

Correlation of	(1)	with '*g*'	0·72
„	(2)	„	0·63
„	(3)	„	0·58
„	(4)	„	0·60
„	(5)	„	0·58
„	(6)	„	0·65
„	(7)	„	0·63
„	sum of tests with '*g*'		0·89

This correlation (0·89) of the sum of tests with the general factor indicates that they have a satisfactory reliability. Assuming that they are satisfactory measures of whatever the general factor is, then the next question is whether this has any relationship to accident tendency.

V. PSYCHOLOGICAL REACTIONS AS ONE OF MANY CAUSATIVE FACTORS.

A man is regarded as accident prone if he has more than a given number of accidents, that number being determined in relationship to the average accidents of the group. The work which has been done in determining the causes of accident proneness has shown plainly that many factors may either singly or in combination lead to this condition. Such factors for instance as health condition, attitude towards job, amount of time worked, hazards of route, home circumstances, luck, etc., as well as abnormalities in psychological reaction. Thus, as psychological reactions are only one of a number of factors leading to accident proneness, it cannot be expected that a correlation of psychological test results with the number of accidents can be very high. In fact it may be said that if such a high correlation were obtained, such a result could not be regarded as reliable. For instance even Farmer, using the best out of a large battery of tests, could not get a higher correlation of test results with accidents than 0·289 (5). The correlation here is of about the same order (0·20).

The effect of the inclusion of uncorrelated factors in lowering correlation between two variables may be shown by the following formula:

$$r_{(x)(x+a+b+\dots n)} = \frac{1}{\sqrt{n}}\,(6).$$

Thus, if we regard x as psychological test result and

$$(x + a + b + \dots n)$$

as being accident proneness, then the size of r depends upon the number of other factors such as $a, b, \dots n$. For instance, if we assume three other factors such as (a) health condition, (b) hazards of route, and (c) number of hours worked, and that these are uncorrelated and of equal weight, then even if the psychological tests are perfectly reliable, r could not be greater than 0·50.

Because of the presence of these various other factors the low correlation coefficient cannot, therefore, be regarded as a suitable measure for determining the relationship between test results and accidents.

VI. RELATIONSHIP FOUND.

Two methods for determining the value of the tests may be used.

In selecting the subjects for testing an equal number of high and low men were taken. In view of the fact that men vary considerably in the amount of operating they do, age, length of service, hazards of route, etc., it was considered advisable to select them in pairs, one high and one low, in such manner that the members of each pair would be alike as far as such conditions were concerned. That is to say, the two members of each pair would be approximately alike in regard to age, length of experience, difficulty of routes operated, etc., but different only in their accidents.

First method. Each high accident man was then studied in relationship to his mate to determine the conditions which might explain his accident proneness. By this means the influence of senility, health, experience, etc., was in large measure eliminated. Thus, psychological condition would not be given as a cause of the accident proneness of a man unless his test record was poorer than that of his mate. The results of these comparative diagnoses are shown roughly classified below.

Table II. *Comparative diagnosis of accident proneness.*

	Major factor	Minor factor	Total
Psychological tests	6	17	23
Health and medical report	16	4	20
Relations to job	8	12	20
Other	13	1	14

This table shows that the psychological tests did differentiate the proneness to accident. It also serves to illustrate the presence of other factors, and the consequent fact that a correlational relationship would be attenuated by their presence. It will be seen that in the majority of cases more than one factor contributes (in fact in some cases three factors might be present).

It will be seen that in only 23 cases out of 43 was the psychological test record of the high accident man the poorer, leaving 20 cases in which the good operators made equal or poorer test records. This fact, while again it shows the reason for low correlation, does not minimize the value of the tests.

The main object of the tests was to assist in the diagnosis of accident proneness. In so far as in 23 cases the test results have thrown light on the behaviour of the individuals concerned they have been valuable. In this respect low total test scores just serve to point to the subjects whose reactions are peculiar. Individual analyses of test results then follow.

It may be stated that there is no significant correlation between any of the four factors given above.

Second method. The 86 subjects were divided into three groups, *A*, *B* and *C*, according as their test records were good, medium or poor. The number of high and low men in each group was then found with the following result:

Table III. *Comparison of test score with accident record.*

Grade on test	High accident	Low accident
A (good)	16	20
B (medium)	19	21
C (poor)	8	2

This table again illustrates the fact so commonly found in the application of psychological tests to industrial situations—namely, that those who are very poor in the tests are the ones who prove the value of the tests—a fact which correlation coefficients hide.

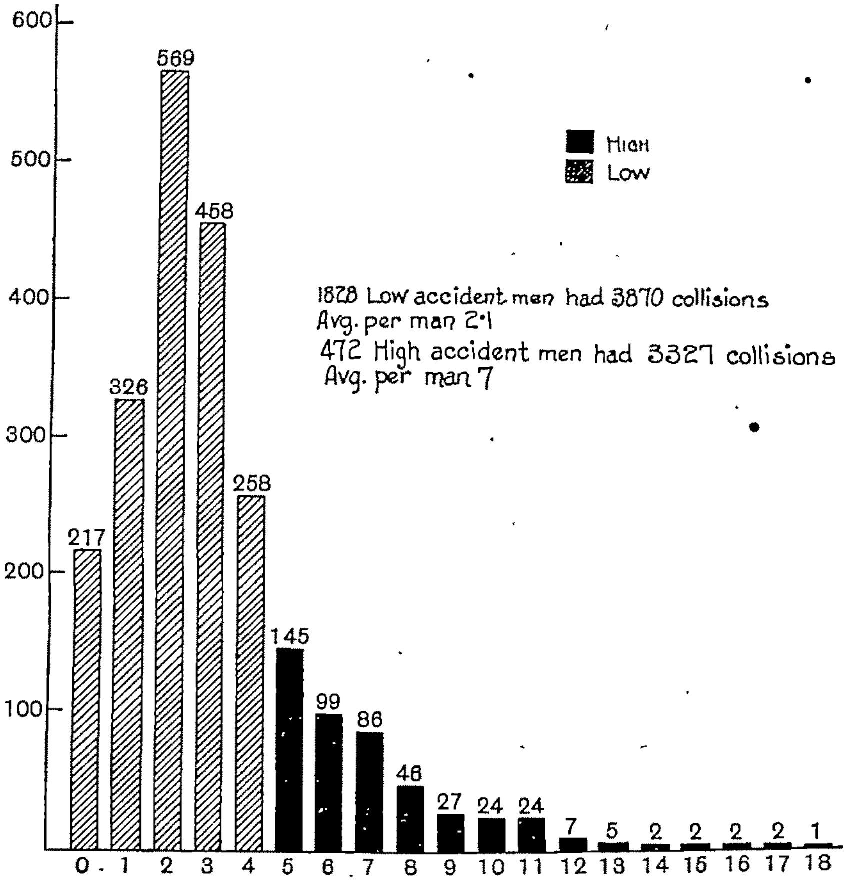

Fig. 1. Chart showing proportion of high-low accident men in Jan. 1928.

The differentiation between high and low accident men of the Grade *C* testees shown above is quite sufficient to indicate that the tests used are related to accident proneness.

It is suspected that coarseness of grouping in regard to age, experience, etc., has influenced these results, so that further work projected will be done with more homogeneous subjects. It is considered, however, that the results herein outlined are sufficiently encouraging to warrant the testing of a further larger group.

VII. SUMMARY AND CONCLUSIONS.

1. The difficulty of diagnosing the causes of accident proneness led to the design and set up of psychological tests, mainly of reaction.

2. Of eleven tests seven proved reliable, correlating 0·89 with '*g*.'

3. No group factors were found present, but only '*g*' and '*s*.' The nature of this '*g*' is unknown.

4. The ordinary correlation coefficient shows no significant relationship between test results and accident record.

5. Two other methods, however, show the value of the tests in diagnosis and the relationship to accident proneness of extremely poor test performances.

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PSYCHOLOGICAL TESTS IN RELATION TO EDUCATION AND VOCATIONAL GUIDANCE[1].

By SHEPHERD DAWSON.

I. Neglect of practical application of psychological tests in Great Britain.

It would be an exaggeration to say that psychological tests have played any significant part in education as it exists in Great Britain to-day. Their principal service, apart from the diagnosis and measurement of mental deficiency, has been to bring home to the mind of all interested in education the fact that children differ in innate mental capacity and that such capacity is distributed like any other natural character. Even in measuring mental deficiency the medical man, who alone is permitted to certify such cases, puts little trust in them, preferring to rely on his own general impressions.

The recognition of variations in capacity may appear a trivial accomplishment, but it marks the beginning of a new outlook in education. To appreciate its importance one has only to remember that last century it was the practice of the Board of Education to allocate grants to primary schools on the number of children who passed the annual examination, the assumption being apparently that all children were capable of a scholastic goose-step and that the teaching was to blame for any failure to keep in line. That practice has been abandoned because it was unjust and unworkable, and the results of psychological testing have confirmed the wisdom of the change and have shown the futility of imposing a rigid curriculum on all and the need for studying children and making the most of their potentialities.

These tests have hardly been used at all, except experimentally, for purposes of school organization in this country. Practically the only tests that have proved useful have been the tests of general intelligence. Tests of imagination, reasoning, attention, memory and all those mental functions which figure in books like Whipple's well-known volume, are at present little more than laboratory curiosities, except in the hands of the vocational psychologist, and perhaps he too is no exception. Nobody takes seriously their immediate results as tests, not even the psychologist. It is practically impossible at present to make any clearly-cut statements of practical value about ability to reason, to attend, etc., from the information provided by using these tests. The records of intelligence tests have been analysed so as to show how intelligence is distributed, how it varies with social position and other conditions; they have yielded a mass of information and raised a crop of problems: but nothing corresponding to this has arisen from the use of other psychological tests.

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[1] This paper was read before Section J at the South African Meeting of the British Association, July, 1929.

II. Reasons for this neglect.

This is due partly to lack of interest on the part of the administrators, partly to the difficulty of finding suitable tests, and partly perhaps to the policy of psychologists, who, realizing the tentative character of the tests and the difficulty of interpreting them, have deprecated their use by any but themselves; but the fault lies mainly with the tests themselves and particularly with the psychology that lies behind them. They smack of quackery. Many of them are published without any indication of either norm or probable error, so that one may use them without knowing the significance of the results obtained. Even when norms are given, they are often based on so few records that they are obviously useless. Sometimes this may be due to a desire on the part of their authors to prevent their misuse, but more often it seems to be the result of ignorance of what a test *is*. To print a set of more or less interesting questions and give them a psychological name is not to invent a psychological test: they must test some psychological function.

Essentially, a psychological test is some short problem or set of problems given, not for themselves, but for the purpose of indicating capacity or ability; for example, the ability to memorize a piece of prose, a list of words or nonsense syllables *might* be taken as a measure of the ability to memorize other things (actually, it would be a very unreliable measure). In some respects it is like the sampling done by the analytical chemist when he examines a cargo of ore or by a prospecting geologist when he makes a boring.

III. Capacity and ability.

Here I should like to make a distinction between ability and capacity. Capacity is a matter of natural endowment; it is aptitude, talent, potential ability. Ability implies more; it implies capacity and in most cases experience and training: it is actual; it is what one can do here and now. Capacity may exist without ability, but not ability without capacity. One may have the potentialities, the capacity, of a mathematician without his ability.

Every test must be primarily a test of ability; but psychological tests claim to test capacity, and therein, it is said, lies their peculiar virtue. Scholastic examinations are more obviously tests of ability, but they are often considered to be tests of capacity; for example, many educationists regard the ability to pass a classics examination as a proof of capacity for dealing with any of the intellectual problems of life.

Although psychological tests are commonly believed to test capacity, there is no reason why some of them should not be explicitly recognized as tests of mental ability. If the ability to reason along certain lines, to memorize certain kinds of material or to observe objects of certain classes can be developed by training—and we all believe they can—and if these abilities can be tested by short tests—which is possible—then such tests are as much psychological as any others.

IV. METHODS OF ESTIMATING CAPACITY.

It is when a test claims to discover and measure capacity that we come up against difficulties. There are several ways in which what is really a test of ability *might* become a test of capacity.

(1) If one can be quite certain that a test is new to all the examinees, then it seems reasonable to suppose that success at this test can be determined only by innate capacity. An example would be a test of musical capacity applied before the examinee had had any sort of training. It is, however, difficult to find such tests, perhaps impossible. Further, if such a test is to be of practical value, we have to assume that people of like capacity are equally able to profit from the same training; but it is just possible that they are not, that they differ in trainability. If they do differ in this respect, then a test of this kind will be of value only if it be accompanied by a test of trainability. This suggests a second method.

(2) The examinees might be given complete training and then tested. Here we assume that differences of ability revealed under these conditions depend solely on differences of capacity. For practical purposes of prognosis this method is obviously useless: but it would be useful for checking the results of other methods.

(3) Finally, the test may be of such a kind that it can be assumed that all the examinees have had equal training or equal opportunities of training. The Binet tests of intelligence are the best example of

this kind. Binet put to children a number of questions about topics which were likely to come within their everyday experience, and assumed that those who picked up the necessary information or acquired the necessary skill at an earlier age than the average child were bright or intelligent, and that those who were slow in doing this were dull: and subsequent enquiry has confirmed his assumption. The danger here lies in variations of opportunity and training. Obviously, a child who has not had the opportunity of using the current coinage, or of buying and selling, or of learning to read and write, is at a disadvantage when he faces the Binet tests. The danger, however, may not be so serious as it appears at first sight, for the social environment of children living in civilized communities differs very little.

Until these methods have been compared, the use of psychological tests must be attended by some uncertainty. The third is the one which has proved most successful hitherto, partly because it has been used more extensively than either of the others, and partly because it has been used with children whose educational training is remarkably uniform (up to the age of 9 or 10 the schooling given to children of all classes is very much the same).

We are specially in need of sets of observations made on the same individuals over a long period of time, for it is only by such observations that one can measure the validity of a psychological test as a measure of capacity. Validity must be distinguished from what is commonly called 'reliability.' Gauging the reliability of a test generally means finding whether it gives consistent results when it is repeated on the same individuals at intervals of a few days. 'Consistency' might be a better name for this. Consistency is not the same thing as finding whether a test measures capacity, for success at a test may depend on some special ability which has been acquired, and, as a method of measuring that ability, it may give quite consistent results without indicating capacity.

Validity can be established only by showing that the relative quality and quantity of the work done at a first performance correlates closely with subsequent work of a similar kind. Comparatively little work has been done on this particular problem, and most of it has been in connection with tests of general intelligence. Several investigators have made a series of observations on the same children extending over a year or more for the purpose of finding whether the I.Q. is constant, and this ratio does measure, or at any rate claims to measure, capacity. Unfortunately, the intervals between the testings have usually been very short, most of them a year or less. They are, however, sufficiently numerous to produce the conviction that the I.Q. is approximately constant during school life.

The common practice of correlating success at vocational tests with success during apprenticeship, as judged by a foreman or manager, is a step in this direction, but usually a very short and uncertain one: the establishment of a correlation between success at a test and a foreman's judgment on an apprentice's ability does not prove the presence of a specific native capacity. Until enquiries like those into the constancy of the I.Q. have been made with other psychological tests, it will not be possible to speak with certainty of their validity as tests of capacity.

V. CONSTRUCTION OF TESTS.

As regards the principles on which the tests have been selected or constructed, some of them seem to be the issue of an accident, others are obviously the result of introspective analysis or popular psychological distinctions. The so-called sample tests, analogous tests, empirical tests, and tests of elementary sensory capacities all imply a certain amount of introspection. The origin of a test is really a matter of little importance; it does not affect its usefulness. Introspective analysis of it is useful, but only as an indicator; the real test of a test is pragmatic—its usefulness—the way it works. This is shown very clearly by intelligence tests. As intelligence is a popular word with an ill-defined connotation, a good intelligence test must grade people in much the same way as they are graded by a competent observer: it is, however, less subject to individual opinion; it is more precise and speedy.

The invention and use of psychological tests has been much hampered by the old 'faculty' psychology, which is still popular in one form or another. The terms used in naming them, intelligence, memory, attention, etc., are embarrassing; they suggest entities which are to be measured, whereas what one really tries to do is to examine and record the results of the subject's behaviour when he performs tasks in which he is supposed to be remembering, attending, or acting intelligently. It would make for progress if we could frankly abandon these popular notions and concentrate on the ability to cope with certain situations. Then we could make out an intelligible programme of enquiry: we could ask how soon the abilities appear, whether they remain constant, whether and how they can be developed, whether they are correlated with one another and with vocational success, whether they are the same in parent and child, how they are distributed among the population, and so on.

There is still much doubt whether the intelligence of adults can be tested as can that of children. It may be true that the average mental age of the American Army as measured by group tests was not more than 13 years, but it is possible that the tests did not really tap their intelligence; for the situations they presented were so strange to the soldiers that the latter could not bring to bear on them what intelligence they had. We have tried in Glasgow, but so far in vain, to find paper tests which are applicable to adults of the labouring classes. Fathers and mothers of these classes get into such a state of excitement at the sight of paper and pencil that they are quite incapable of responding sensibly: one of them remarked, "You should ask our wee Johnnie to do this: he's at school." Their mental age, as measured by such tests, is almost invariably lower than that of their own children. It is, of course, possible that the race is improving, but not at this rate; it is more probable that, having been so long away from school, these people have lost the scholastic habits of thought which are necessary for success in these tests. It is not even certain that these tests are very reliable even with educated adults, for the correlation of their results with scholastic and academic success is often very low.

VI. Their practical value.

So far we have been considering psychological tests from the impersonal, scientific, and logical point of view; but they have their practical, clinical side. Practice involves more than logic: it involves insight, imagination, and the ability to 'sense' the effect of subtle conditions—what is commonly called 'intuition.' While there is much to be done to put these tests on a firm scientific footing, their usefulness is beyond question. Mental testing is like a good deal of medical practice: it is practical and empirical rather than scientific; it is a useful makeshift which must be retained until something better appears.

General intelligence tests have come to stay: no one who has used them seriously questions their usefulness. The others are, on the whole, at present at any rate, more useful for what they suggest than for what they actually tell you. This is particularly true of performance tests, as one may gather from Earle's *Report on the Use of Performance Tests of Intelligence in Vocational Guidance.* After reading this report one is left with the impression that, as tests of intelligence, they are very imperfect, that they test special abilities, and that their principal use in vocational guidance is the opportunity they give to an acute observer for forming an impression of the temperamental traits and general behaviour of the examinees.

The same impression has been given by the use in clinical work not only of performance tests, but of tests of more obviously specific abilities, like memorizing, forming associations (substitution test), and quickness of perception (cancellation). In the Glasgow Educational Clinic, where children are examined who have been referred to it on account of abnormalities of behaviour or inability to make normal progress in one or more of the school subjects, it has been our practice to make fairly full case histories and to supplement these by the results of psychological tests. The most generally useful of these is the intelligence test. Often nothing more is required: sometimes it shows, for example, that a child's backwardness in his school work is just the consequence of a natural dullness, which has escaped the attention of the teacher, especially when the child is docile and hard-working. Frequently, misdemeanours, such as bullying and truancy, are the obvious result of inability on the part of the child to keep pace with his fellows. The general intelligence test provides an opportunity for examining many aspects of a child's mind and for making observations on his temperament and general behaviour. The special tests, *i.e.* tests of special abilities, are useful for following up suggestions gained in the course of the more general examination.

Mental tests do not yet give a picture of the human mind that is at all comparable with the picture given by a set of physical and physiological measurements. There seems, however, reason to hope that when they have been applied extensively and their results subjected to careful statistical and psychological analysis, they will take their place with such measurements. Then they will form an integral part of our educational system, and it is possible to some extent to predict the part they will play. They will play an important, but not very considerable part, for they will concern problems of organization rather than of teaching, and teaching is, after all, the biggest thing in education. Their importance will be most obvious at, but not entirely confined to, the beginning and the end of a child's school career. For purposes of classification they will supplement scholastic tests, and by showing what can be expected of a child they will give a fairer estimate than is now possible of the work of both teacher and pupil.

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A NOTE ON THE RELATION OF CERTAIN ASPECTS OF CHARACTER TO INDUSTRIAL PROFICIENCY.

BY ERIC FARMER.

DURING an investigation for the Industrial Health Research Board I watched several hundred boy apprentices do certain psychological tests, and I endeavoured to judge some of their characteristics by the way they did them. I quickly found that differences in the methods of doing the sensori-motor tests used in the investigation were not sufficiently marked to be the basis of a character assessment, and so I confined myself to judging the subjects' characters by the way they did a performance intelligence test (cube construction). The object I put before myself was to note the various ways the boys did the test, and to learn from that the proper categories to divide them into, and I definitely avoided using any preconceived psychological notions of my own.

The first group contained 259 boys, and I made very full notes of the methods employed. When these were analysed, it was found that they were far too varied to permit of their being grouped into well-defined and exclusive categories so that they might be compared with the boys' subsequent trade proficiency, which was the object of the experiment. Certain definite characteristics showed themselves, however, and it was decided in future to pay attention to these only, and to divide the subjects into exclusive categories according to what appeared to be their dominant characteristics. The categories were as follows:

The controlled type. The chief characteristic of this type was their complete mastery of the situation. They did not appear to think ahead, nor were they worried by the test in any way. They did it without any concern, hurry, or manifestation of interest.

Thinkers. This category was confined to what appeared to be abstract thinkers. They seemed to think ahead and have a definite plan in their mind before doing the test.

Good workers. This category consisted of the conscientious plodding type. However much they were baffled by the problem presented they continued to work at it. Sometimes they broke down all they had done and started again. They were often worried by the test, but they never thought of giving it up and they learnt by experience.

The inept. This category was composed of those who had no idea whatever how to do the test. (In my original notes I called them 'fools,' and that is really the best description, for their methods seemed to be utterly irrational.)

Miscellaneous. Into this category I put those about whom I was uncertain, or who failed to impress me with any outstanding characteristic. The results obtained from the first group of subjects showed that it was better to have such a category rather than force every one into one of the positive categories.

Two further groups of subjects were then observed, using the modified technique. One group contained 259 subjects and the other 347. The two groups were entirely similar. Separate analyses of their character assessments showed no differences between the two groups, and so their results are presented together.

The industrial proficiency of the subjects was measured by a practical examination, part of which was given after one year's training and the remainder after three years' training. The examination was very conscientiously carried out, partly by those who knew the boys' work during training and partly by outside examiners. The curve of distribution of the examination marks was of the normal saddle-backed type.

Table I. *Scores in industrial proficiency examination.*

Controlled	Good workers	Thinkers	Inept	Miscellaneous
460	454	449	431	435
	455		433	

There is little difference in the marks scored by those in the first three categories, or between those in the last two, but the score of the first three is higher than that of the last two categories. In any practical application of such a method of character assessment the first three categories would certainly be accepted for employment and probably the latter two rejected. This would mean that 57 per cent. of the applicants would be accepted and 43 per cent. rejected. Dividing the subjects in this way, a biserial correlation was obtained between the character assessments and the subjects' industrial proficiency which turned out to be $0{\cdot}227 \pm 0{\cdot}037$.

It has already been pointed out that the character assessments were made while the subjects were doing the cube construction test and, although I did not know whether the subjects would fail or succeed in the test till after the assessments were made, yet it was clear that there would be a high correlation between the two ratings, and the correlation coefficient obtained by the fourfold method turned out to be 0·946 ± 0·042.

The biserial correlation coefficient between the cube test and industrial proficiency was 0·217 ± 0·037. This correlation is of the same magnitude as that obtained by means of the character assessments, so that the same ends could have been gained by using the test alone and dispensing with the character judgments.

Before being selected for training the subjects were given a written examination, and by dividing the results of this examination in a similar way to the character assessments (*i.e.* accepting the best 57 per cent. and rejecting the rest) it was possible to work out a biserial correlation between the entrance examination and the subjects' industrial proficiency and the coefficient yielded was 0·149 ± 0·038. The same method was applied to a group of objective tests[1] (which included both the entrance examination and cube test but no character assessments) and the biserial correlation coefficient was found to be 0·313 ± 0·035.

It may be said that in a practical application of character assessments for vocational selection only those in the inept category would be rejected, the data were therefore re-examined by the methods described above but on the assumption that only 31 per cent. (the number in the inept category) would be rejected. This yielded the following biserial correlation coefficients.

Character assessments and industrial proficiency ...	0·209 ± 0·037
Entrance examination and industrial proficiency ...	0·188 ± 0·037
Objective psychological tests and industrial proficiency	0·370 ± 0·034

SUMMARY AND CONCLUSION.

The results obtained in this experiment by character assessments were better than those from the entrance examination, equal to those from the cube construction test and inferior to those from the group of objective tests suggested as a result of the investigation of which this particular experiment formed part.

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[1] "A study of personal qualities in accident proneness and proficiency." By Eric Farmer and E. G. Chambers. *Industrial Health Research Board, Report* 55. H.M. Stationery Office, 1929.

An obvious criticism of this experiment or of any experiment of a similar kind is that the character categories were not well chosen or the judgments not properly made. It may be that other workers, using different categories, will get better results. In favour of the present categories it may be said that they were chosen as a result of the analysis of very full character assessments of 606 subjects, and they were suggested by observing the actual differences between the methods employed, and were not the result of preconceived psychological notions. There is some value in the work, inasmuch as fairly large numbers of subjects were observed and their character assessments compared by accurate methods with an objective criterion. Some of the work that has been done in this field ends by giving a description of the subject's character or comparing the characteristics revealed while doing a test with other characteristics of the subject known to the experimenter.

Positive results obtained by this method cannot do more than show that certain characteristics revealed while doing a test are also manifest in other situations. It cannot show how far such characteristics are helpful or otherwise in an occupation, unless they are compared with an objective measure of success in the occupation itself.

It is idle at the moment to discuss the relative value of character assessments and objective psychological tests as means of vocational prognosis. This relatively new branch of experimental psychology must be given time to develop, and there are few fields of research which offer better opportunities for the introduction of new methods in experimental technique.

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THE PHENOMENA OF ATTENTION AND VISUALIZATION IN THE PSYCHOLOGY OF MUSICAL APPRECIATION.

BY P. E. VERNON.

(*From the Cambridge Psychological Laboratory.*)

I. *Introduction. Method of obtaining data* (pp. 50–51).
II. *Physiological effects of music and wandering of attention* (pp. 52–55).
III. *Emotional interpretations and the theory of visualization* (pp. 55–57).
IV. *Visualization phenomena* (pp. 57–63).
V. *Summary* (p. 63).

I. INTRODUCTION. METHOD OF OBTAINING DATA.

MANY investigators in the psychology of musical appreciation have noted the free trains of thought and visual imagery which constitute a large part of their subjects' methods of listening to music, or, as Vernon Lee[1] aptly puts it, 'hearing' music. But there is a great deal of confusion still as to the scope and psychological nature of these phenomena. Though they and synaesthesia[2] clearly merge into one another, yet it seems to me desirable to draw definite distinctions between the three, on the basis of results obtained with a larger number of subjects of varied musical capacities than have probably been used before.

The musical responses of 104 people were investigated between 1927 and 1929, 50 of them being examined by elaborate questionnaires, given in person, and 71 attending one or more of three experimental concerts[3]. The most valuable data were obtained when conditions were partly controlled and made as similar as possible to those of an ordinary concert. I also tried employing an entirely free situation and, at another time, an unannounced programme played by hidden performers. But there are strong drawbacks to the uncontrolled and to the strictly controlled musical situation, which I have discussed elsewhere[1]. Thus the programme is given to members of the audience in the usual way and the following directions are mimeographed for them and explained in a preliminary talk.

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[1] V. Lee, "Varieties of musical experience," *North Amer. Rev.* 1918, CCVII, 748–757.

[2] Cf. P. E. Vernon, "Synaesthesia in music," *Psyche*, 1930, X.

[3] My rather belated thanks are due to Dr H. Banister, Mr D. D. Arundell, and fourteen other Cambridge musicians who helped me greatly in arranging and performing at these concerts.

> After each musical item (and, if you like, during the music) you are asked to try to write down as fully as possible *what you have been thinking about while you were listening*. Mere criticisms of the music are not desired, but an account of the thoughts and feelings, etc., that you actually experienced.
>
> First try to summarize your impressions under a few general headings such as:
>
> A. Anything not directly related to the music, e.g. surroundings, audience, wandering of attention to extraneous matters, unnaturalness of the situation or introspection.
>
> B. Visual images (i.e. mental pictures) of scenes, colours, patterns, etc.
>
> C. Tendencies to action, any kind of bodily movements, response to rhythm.
>
> D. Interest in technical aspects of the music, style, interpretation, etc.
>
> E. The goodness or badness, or value of the music.
>
> F. Feelings or emotions aroused in you, such as excitement, restfulness, amusement, sadness, etc.
>
> G. The aesthetic or emotional appeal of the music to you, or the musical pleasure you obtained from it.
>
> All of these are not likely to apply to you; if so, just use the ones that seem to you necessary to include all your thoughts and feelings while listening.
>
> Indicate how much you attended to, thought or felt each one in each piece of music by writing down the number of the piece, then A and either 0, 1, 2 or 3 marks, where 0 means that this factor played no part in your listening, and 3 means that it played a very important part. 1 and 2 (and halves if you like) are intermediate grades. Do the same marking for the other headings, B, C, whichever you choose.
>
> Then during the rest of the interval try to amplify these marks by describing as fully as possible the various details of what you thought about, felt or noticed while listening.

Chamber music of a high standard was used throughout, such as a Mozart Trio, classical and modern pianoforte works and violin solos, songs by Elizabethan composers, Brahms, etc.

In working up the results it was useful to be able to compare responses with the musical-ness or musical level of the subjects. This quality was determined by reliable ratings on a 0 to 10 scale[2]. The most musical subjects, University musical instructors and the like, are called Aa, Ab, etc.; the next grade, highly musical amateurs rated as having a musical level of 9, are called Ba, Bb, Bc, etc., and so on down to the least musical grade, rated 0, called La, Lb, Lc, etc. The latter were usually unmusical psychologists. Thus the first letter assigned to a subject indicates with fair accuracy how musical he was, in the following discussion.

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[1] P. E. Vernon, "Non-musical factors in the appreciation of music," *The Musical Times*, Feb.–April, 1929, 123–124, 227–228, 320–321, and "Method in musical psychology," to be published shortly in *Amer. J. of Psychol.* 1930, XLI, XLII.

[2] Cf. P. E. Vernon, "A method for measuring musical taste," to be published in the *J. of App. Psychol.* 1930, XIV.

II. PHYSIOLOGICAL EFFECTS OF MUSIC AND WANDERING OF ATTENTION.

I will state some general theoretical conclusions first before illustrating them and presenting the introspections and records upon which they are based.

The two phenomena, visualization and wandering of attention, may both be considered without any reference to music as an art or as involving any intellectual discrimination. They are primarily the responses of the musically uneducated, the results of allowing music to act upon them almost automatically, to sink in without their attempting to analyse it actively. Free trains of thought are, however, less evolved in the musical scale than visual imagery. They are based on the primitive, reflex physiological effects of sound, while the true visualization is essentially an emotional response, derived from what may be termed a subjective analysis of the actual music.

The stimulatory effects of noise and especially of musical noise on bodily metabolism, as investigated by Féré, Tarchanoff, Weld[1], etc., and on animals and savages are too well established to require resummarizing[2]. Music seems to increase physiological activities generally, alters and usually accelerates pulse and respiration, lowers sensory thresholds and diminishes muscular fatigue in various directions. Hence it has been put to various therapeutic uses with some success. Educated people under hypnosis, it has been asserted, regress to more primitive and overt types of musical response, the bodily expression of the emotions being very marked. Head and Holmes' thalamic patients showed similar involuntary and intensely emotional susceptibility to music[3]. Some of my subjects recalled analogous uncontrolled responses of their childhood, while even babies wave their hands spontaneously in time to rhythmic music. It is hardly rash to suppose that much of the appeal of modern dance music consists in the euphoric effects of its rhythm and tone colour, since it is seldom attended to *per se*. Except among those who, disapproving of it, inhibit it, it seems to stimulate all the muscles of the body, to produce empathically the well-known 'itching of the feet,' and so genially exercises the whole organism without the violent and overt movements which were characteristic of the older type of dances. The present popularity of Bach may well be due to much the same process. Thus, subject Bd records at one concert that he generally only appreciates familiar music, but "I can always enjoy Bach—a good dance—at first hearing." As is well known, singing helps sailors and primitive peoples when engaged on rhythmical tasks. Howes[1] describes some of the effects on memory and other mental and bodily processes of purely rhythmic stimulation, also the physical and moral improvements produced by folk-dancing. Presumably this is also the underlying idea of Dalcroze Eurhythmics. Among my subjects Gf noted that he can work much better the day after a concert that he has enjoyed. Most of those who answered my questionnaire, who usually attend to music actively, admitted that music in a restaurant or cinema, which is not attended to, might act as a pleasant background apart from its good or bad musical qualities. Dk added that it produces a pleasurable increase of 'general emotional tone.' Hostesses similarly exploit these stimulatory effects of music 'to get people to talk.'

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[1] H. P. Weld, "An experimental study of musical enjoyment," *Amer. J. of Psychol* 1912, XXIII, 245–308.

[2] These are fully described by C. M. Diserens, *The Influence of Music on Behavior*, 1926.

[3] H. Head, *Studies in Neurology*, 1920, p. 560.

Several instances of its ideoexcitatory powers may be cited. According to Delacroix[2], Darwin complained that music actually fatigued him by making him think too vigorously about his researches. The same writer quotes Stendhal: "Toute musique qui me laisse penser à la musique est médiocre pour moi." Tolstoy seems to have been similar. Bg recorded that, though he seldom allows his thought to wander while supposed to be listening, yet "it is the future rather than the past that my mind dwells upon during music." Fg says that on occasion music produces a pleasant train of thought that is clearer and quicker than usual. Hb is merely bored by bad music, attends to moderate music, and is definitely stimulated to thought by good music. Ic wrote, "Music acts as a stimulus to my mind which flutters from this thing to that."

What then is the nature of this automatic, stimulatory effect of music on the organism? It is necessary to protest against the perpetuation of early nineteenth-century physiology by some writers such as Hadow[3], who continue to state that music "directly affects the auditory nerve," usually *via* the labyrinth! To this they assign the bodily and therapeutic results.

Actually one cannot find anything much more definite to substitute for this assertion. Presumably music in some way harmonizes or regulates neuromuscular and organic processes generally, processes which are themselves periodic. By providing a background it induces a kind of hypnotic state and seems to eliminate many of the tendencies that distract from concentrated bodily or mental activities, so lessening fatigue. These facts, it may be noted, are of considerable importance in the psychology of attention, being directly opposed to the notion of its limited, quantitative nature.

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[1] F. Howes, *The Borderland of Music and Psychology*, 1926.

[2] H. Delacroix, *Psychologie de l'Art*, Essai sur l'Activité Artistique, 1927.

[3] W. H. Hadow, *Collected Essays*, 1929.

Various lines of evidence tend to show that the ideoexcitatory effects of music are more subordinated in the more musically educated. It is of course difficult to compare different subjects' ratings of their lapses, yet the following considerations prove that these ratings are fairly reliable and that they agree with general expectations. The average marks given to each item under category A (irrelevant thoughts) varied, in one concert, in exact inverse relation with the sum of the marks given to the other more musical categories. In another concert where the items were of a more homogeneous nature, the A marks varied directly with the length of each item but for one exception, a pianoforte improvization in the so-called 'ultramodern' style. This naturally produced high markings for B, C, D and F categories, and a correspondingly low A mark.

Taking now those members of the audience who wrote down 0 to 1 as their average A mark for different items, those who wrote 1 to 2 and 2 to 3, we get groups of 12 per cent., 74 per cent. and 14 per cent. who wandered little, moderately or irregularly, and most of the time, respectively. The average musical level ratings of members of these groups were 7·7, 5·7 and 2·2. Similar figures were obtained in the second concert. In my questionnaires 20 per cent. of subjects admitted that wandering of attention was pleasurable to them, while 14 per cent. stated that they always try to concentrate on the music, analysing it actively. Their average musical levels were 4·3 and 7·7 respectively. These differences indicate a large amount of overlapping, but they are fairly significant statistically, being two or more times their P.E.'s. It is obviously untrue to say that all the musical listeners attend and that all the unmusical indulge in free trains of irrelevant thought. Probably the stimulatory effects are present in all types of listeners, but are not so much a source of enjoyment in themselves among the more musical. Any musician would admit that his attention lapses if the music is very bad or boring; that personal associations with previous performances and more or less distracting thoughts of historical and technical interest tend to enter continually and may be very pleasurable. Many subjects noted that their attentiveness varied with mood, fatigue, the type of music, etc. Some are more actively analytical towards classical styles, less towards Romantic and Impressionist. Some attend far better when they are already acquainted with the composition, others analyse new pieces and later allow them to 'sink in.'

But the nature of this analytical attention, culminating in what Sabaneev[1] calls complete 'tonal consciousness,' is outside the scope of this article. It seems that one must allow that musical people develop a capacity whereby they voluntarily repress other impressions. Eb, for example, was quite certain that when listening (with his eyes open) he did not see his surroundings at all and that he had no visual images, *i.e.* that his experiences were solely auditory and affective. Several subjects suggested that they have a musical conscience, a 'guilty feeling' when they have wandered from the music. Psychologically this conscience is probably a sentiment, in McDougall's sense. It motivates also the inhibition of the more obvious aspects of the music so that they hear the less obvious, such as the inner parts.

III. EMOTIONAL INTERPRETATIONS AND THE THEORY OF VISUALIZATION.

When specific emotions and visual images are produced, we reach a more evolved and differentiated stage than the previous physiological one. It is as if the reflex organic reactions had become qualified or interpreted, though still without any conscious objective analysis. First there is the so-called sensuous effect of timbre apart from pitch or duration relations. Thus, according to Sabaneev's experiments, there is no clear distinction among the unmusical even between pitch and intensity; a note that is either higher or louder than another note will be called clearer or sharper. Sherman[2] demonstrated that an unseen singer, by means of a single note of standard pitch and intensity, could convey emotions which corresponded well with the reports of a number of listeners. Presumably an explanation along the lines of Darwin's theory of facial expression of the emotions would hold here.

Next, by its resemblance to the sounds and motions of objects, music may suggest hurry, rest, ascent or descent, solemnity, mystery, martial tendencies, etc. One must allow that these motor responses are capable of evoking the emotion or mood which they characteristically accompany, as in Ribot's classical theory of emotional memory. Other affective states are more or less permanently attached to certain types of music; sadness to the minor key, for example, is most common. In the cinematograph industry far more exaggerated uses are made of this aspect of the musical response. Cinema producers frequently use music to intensify the expression of actors' feelings in 'close ups' and emotional scenes. Writers of music to accompany films and cinema orchestra directors assume that specific pieces of music can induce specific emotions in the majority if not the whole of their audiences, though to a psychologist such an assumption seems extravagant[1]. The only confirmatory experimental evidence for it that I have found is that of Farnsworth and Bennett[2], who played music to a number of students engaged in imaginative freehand drawing. The resulting pictures were said to exhibit a considerable similarity in general atmosphere and feeling. In many concert programmes extremely fantastic emotional interpretations are read into the music even by such musicians as Cortot and Schweitzer[3], although most of the evidence goes to show that no two people appreciate in identical fashion nor spontaneously make identical interpretations. Thus Gilman[4] discovered far less agreement than he had expected among people who listened to musical items, the titles of which were unannounced but which, he had presumed, portrayed certain emotions. However, although the exact amount of community of affective responses is unknown, yet it is obvious that emotional interpretations constitute a very common mode of musical appreciation. Some sort of representational meaning is assigned even to compositions of the most abstract intentions. Composers who send in short piano pieces to publishers are usually asked to invent titles for them, though they were not originally meant to represent anything, otherwise 'they will not sell.' The type of person who is completely cold to all music except funeral marches is by no means rare; William Archer was an example. In fact many investigators have neglected practically all intellectual and aesthetic responses in favour of these uncritical emotional reactions. Gatewood[5], for instance, concludes that the more emotions a piece of music arouses, the more is the pleasure derived. Similarly Schoen and Bingham[1] find the degree of enjoyment of music to be directly proportional to the intensity of 'mood effect' produced, the commonest moods being rest, sadness, joy, love, longing and reverence. Other things being equal, they say, music which produces the strongest and most diverse emotional effect is the most appreciated. Though few musicians would agree with these conclusions, yet it is clear that they hold for a very large proportion of listeners, and it is these emotional interpretations which I consider to be the basis of visualization phenomena.

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[1] L. Sabaneev, "The musical receptivity of the man in the street," *Music and Letters*, 1928, IX, 226–239.

[2] M. Sherman, "Emotional characteristics of the singing voice," *J. of Exp. Psychol.* 1928, XI, 495–498.

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[1] For example, in Day and Hunter's catalogue of such music one finds pieces advertised to express the following: defiance, evil intentions, not guilty, torture of the soul, chastity, comic conversation and Satamo fury.

[2] Quoted by M. Schoen, *The Beautiful in Music*, 1928.

[3] A. Schweitzer, *J. S. Bach* (translated by E. Newman), 1911.

[4] B. I. Gilman, "An experimental test of musical expressiveness," *Amer. J. of Psychol.* 1892, IV, 558–576; 1893, V, 42–73.

[5] E. L. Gatewood, "An experimental study of musical enjoyment," essay in *The Effects of Music*, ed. by M. Schoen, 1927, pp. 78–120.

The distinction which I wish to draw between ordinary wandering of attention and visualization is as follows. The former may often take the form of elaborate visual fantasies, possibly with the music or its title as a starting-point, but bearing no direct relation to it other than that due to its ideoexcitatory and hypnotic effects. But the true visualization is a kind of day-dream stimulated directly and continuously by the music as it proceeds and based either on the emotional flux or the physical responses aroused by the music. The two types of response naturally shade into one another. My theory as to the nature of the true visualization may be stated shortly and then illustrated. It is closely analogous to, if not almost identical with, the ordinary dream which, in absence of higher cortical control, represents or translates into visual terms various emotions (the latent content) and physical stimuli such as external noises, cold, indigestion, and the like.

IV. VISUALIZATION PHENOMENA.

There have been several good discussions of the phenomenon. Weld[2], Vernon Lee[3], Myers[4] and Delacroix[5] have described a visualizing type, while R. McDougall[6], as early as 1898, gave an excellent and elaborate account of his own experiences. It will be worth while quoting a few of the introspections of my best subject, Ld (though McDougall's are perhaps even better). They take the form of detailed images, scenes or dramas that are watched as if at a theatre, or rather, since the subject often takes part, as in a dream. They usually continue the whole time the music is going on and no other impressions seem to come into consciousness. While hearing the Mozart Trio for violin, 'cello and pianoforte, he wrote: "I am as a soul without a body floating above the sea; the sea is rough and smooth and little waves are lapping against the side of the boat, from time to time bigger waves come. They come fairly regularly. Now I am on the edge of a cliff and the waves are rolling in on the white chalk below, there is a little ledge against which the little waves in between the bigger ones babble cheerfully. Above me the gulls cry loudly, and not far away a peewit is circling and calling above his nest...."

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[1] M. Schoen, *The Beautiful in Music*, 1928.

[2] H. P. Weld, *op. cit.*

[3] V. Lee, *op. cit.*

[4] C. S. Myers, "Individual differences in listening to music," this *Journal*, 1922, XIII, 52–71.

[5] H. Delacroix, *op. cit.*

[6] R. McDougall, "Music imagery," *Psych. Rev.* 1898, v, 463–476.

Again, in a piano duet by Casella of an extremely cacophonous and rhythmic nature: "...There are men marching down Downing Street" (Cambridge), "they turn into Free School Lane and then down St Botolph's Lane. They are French chiefly, and there are native troops among them, they go onwards in a continuous mass, I am among them and above them marching. Now I can see the foreign legion on the edge of the desert...." A significant remark is made when listening to a later item: "I have been too much awake to appreciate this." In fact the essential state for such visualizing seems to be a kind of somnolent, hypnotic one, which some music is very effectively able to produce. Ld's introspections must have been almost in the nature of automatic writing, since they form a continuous piece of quite poetic prose, written during the music and of a different style from those written when he woke up and recorded: "My soul has returned to my body. The girl opposite has got her eyes shut and looks very foolish. How much longer are they going on for. What a lot of rot I have been thinking." In fact his only other remarks besides his visual dramas concern the performers, the audience or himself, and are almost wholly irrelevant to the music.

La is almost as interesting, and being psychologically trained writes clear introspections. Full enjoyment for him was "a dreamy state in which the instruments and motions (of the music) would seem to play on a complex emotional instrument localized in the body cavity." A piano piece by Purcell calls up, "Dancing country people in gay costumes and in bright spring landscape with flowers." The modern improvization he thought of as "A feminine piece. A woman of volcanic passions and infinite capacity for tenderness and sadness; a woman of intellectual power denouncing a faithless and small-souled lover in a sombre setting." Similar fanciful meanings are given to every item. This illustrates, also, the very close interdependence of emotional interpretations and images.

McDougall believed that his day-dreams while listening arose *via* the emotional response and could often be traced as associations with particular emotional memories or moods that were evoked by the music. Again, Myers found that the grossly unmusical, in whom the music aroused no emotion, were those who least indulged in imagery. My data are inadequate to confirm this, but it illustrates well the distinction I drew between irrelevant trains of thought and affective interpretations. There is a definite indication of a relation between emotion and imagery in the average marks given to different items for the B and F categories.

An even closer parallel appeared, however, between fatigue factors and amount of visualization. Both the average B marks and the proportion of the audience who mentioned imagery increased almost regularly throughout the evening. Naturally one cannot say how far this is due to the nature of the music, but in confirmation of the influence of fatigue we have McDougall's introspections. Also my subject Fc said that visualization only occurred with him in highly emotional music, when he was tired and carried away by the music; and now that he is more musically educated it seldom happens.

That an analytic attitude is incompatible with visualization has already been shown by Myers. In the Casella duet and the modern improvization, many of the audience gave up any attempts at analysing what they heard with the result that visualization was far more prominent. Those who tried to attend to the discords and form as such make almost no mention of images. Bg recorded that he practically never visualizes but did so in the Casella because it was technically meaningless to him. He regarded it as an attractive noise and saw fairies dancing, druidical rites and a native tribe's war dance. In answer to a question, Gf said that some composers call up "attractive visions of ideal scenes," but these disappear as he gets to know the music better and attends to it more actively. A very interesting point is that his images are much intensified by the social influence of sitting with his fiancée. Several others noted that they only visualized when the music had a definite suggestive title, words or programme (in the technical sense), and usually listen more abstractly, forgetting the interpretation, the better they know it. Delacroix gives vivid introspections of a typical visualizer who concludes: "Je n'aimais pas la musique pour elle même, mais...autant qu'elle m'avait laissée rêver." But later she received more musical training and then found that intellectual listening supplanted visualization.

The interdependent development of images and physical character istics of music such as form, rhythm and phrasing was noted by Weld. The fantasies were not, he concluded, stories made up to fit the music, but were spontaneously and involuntarily stimulated by the unfolding of the various features of the music. Again, my subject Ek listened to a Bach Gavotte without being told the name, and saw a number of bewigged and beautifully dressed seventeenth-century people dancing gracefully in time with the music. When Mendelssohn played a Bach Suite to Goethe (a highly unmusical person, it may be noted) the latter said, "It brings before me a procession of great personages in gala dress, descending the steps of a gigantic staircase[1]." Hence also there seems to me to be a distinct onomatopoeic resemblance between my first quotation from Ld and the soothing charm of the Mozart, and between the second and the dynamic dissonances of the Casella. It is interesting to observe too how the unusual features of the latter are rationalized, as in typical dream translation, by appearing as foreign soldiers. Actually one-fifth of the audience employed similar symbolizations of the barbaric and martial qualities of the music, visualizing native war dances, wooden or foreign soldiers and the like, although the title of the piece as announced was "Polka."

With regard to suggesting specific scenes, Gilman, Weld and Downey[2] obtained negative results, except when listeners were told the title of the piece. The latter actually evoked vivid images among her subjects of hunting when playing a piece meant to represent "In a Clock Shop," which was mistakenly announced as "A Hunting Scene." Similarly Wells[3], playing ten unknown gramophone records to an average of nineteen people, asked them to identify which of the ten names belonged to which. On an average only six people identified the name of any one piece correctly, but seventeen were right in a piece called "Longing for Home," and nine in the Siegfried Funeral March. Many of the bad errors of identification were also fairly obvious; for example, nine subjects mistook the "Ride of the Valkyries" for the "Devils' March." He concludes that music tends to suggest very different images to different listeners, even from those intended by the composers.

A summary list of the images aroused by the modern improvization is interesting and shows the degree of consistency possible, when no kind of title nor representational meaning is given beforehand.

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[1] Quoted by A. Schweitzer, *op. cit.*

[2] J. E. Downey and G. E. Knapp, "The effect on a musical programme of familiarity and of sequence of selections," essay in *The Effects of Music*, ed. by M. Schoen, 1927.

[3] F. L. Wells, "Musical symbolism," *J. of Abn. Soc. Psychol.* 1929, xxiv, 74–76.

Bf: Fountains (by auditory association with the style of Ravel's "Jeu d'Eaux").
Df: Chord blocks and vague colours (really a synaesthetic rather than a visualization phenomenon).
Di: (Reminded of) a child in a bad temper, no control over its feelings.
Dk: Puck going round the earth in forty minutes (by auditory association with Debussy's Preludes).

Thus there seems to be no true visualization above the musical level of 6.

Ef: Chaos, earthquakes, other worlds fighting, then a quiet spirit.
Eg: Busy London thoroughfare, traffic.
Fd: Utterly meaningless blue-green shapes running into one another suddenly, hit by strong reds (probably synaesthetic).
Fi: (Reminded of) an iron foundry.
Gb: Representation of chaos (no details).
Hc: Puff, puff of an engine starting.
Ii: A madman having fits, general violence, wildly thumping hands, etc.
Kd: Sea breaking on rocks.
Ki: General ideas behind futurism.
La: A woman of volcanic passions, etc. (see above).

One might be tempted to regard visualization as the response of those whose auditory imagery is so poor that they tend to interpret what they hear in emotional and visual terms; Dalacroix suggests this. From my results I should say that it is only true in the limited sense that the better musicians who usually have good auditory imagery also do not often visualize while listening. But there were plenty of cases of well-trained musicians who never visualized but who also possessed no true auditory imagery. Nor is the response necessarily a characteristic of those who have good visual imagery in ordinary life, any more than are night dreams. Like McDougall, several visualizers had poor visual imagery normally, and among questionnaire subjects I found it impossible to establish any significant relation of this kind.

There remains the matter of its distribution and frequency. Most writers have considered visualizers as a distinct and common type, though Myers allowed that almost everyone visualized more or less often. I find that ordinary visual associations, which can hardly be distinguished from mere wandering of attention, occur in the majority of subjects. My concert audiences were a rather highly selected group, yet half of them mention imagery at some time. But the true visualizer, who alone can be called typical, is decidedly rare. His two main characteristics are, first, that his imagery, like that of McDougall, follows the music and is based on it throughout (what I called above subjective analysis). Secondly, the imagery is enjoyed for its own sake and constitutes, in fact, almost his sole source of pleasure. Again, of course, intermediate cases are commoner, *i.e.* subjects who respond in this way for short periods. So that no strict dividing line can be drawn between attention lapses, trains of irrelevant and relevant associations and fantasies and the extreme type of day-dreams. At two concerts not more than 10 per cent. seemed to enjoy the imagery *per se* at all frequently, while scarcely 5 per cent. depended on it for their main method of appreciation. More adequate introspections might easily have elicited more, for, like Myers' subject M, there were some people who supposed themselves to listen wholly abstractly but who, in the actual concert situations, produced some quite elaborate imagery. Again, my subjects were all highly educated, intelligent people; one might expect more of this passive day-dreaming among those who are not University graduates or undergraduates.

It is worth noting that no marked sex differences appeared.

Vernon Lee divides her 200 odd subjects into musical 'listeners' and unmusical 'hearers,' saying that the latter always weave some kind of emotional or visual drama into the music, appreciating it because they find "a meaning or a message in music, something beyond itself." At my concerts the average musical level of those who mentioned imagery was 3·6, while of my questionnaire subjects one-third, of musical level 7·2, assured me that they never visualized at all. I have no intention of discussing here the rôle of emotion in aesthetic appreciation, but there is certainly a tendency for the musician to regard the appearance of concrete emotions and interpretations as irrelevant or as a sign of the music being definitely bad, though there are many exceptions (such as Cortot and Schweitzer, as already mentioned, and Howes[1]). From questionnaire answers I found it possible to classify 19 per cent. and 28 per cent. into Lee's 'hearers' and 'listeners,' with some distortion. One example of each class may be quoted. Ka says, "I much prefer music of love and sadness as it gives me such a restful feeling and quite takes one out of oneself." Hence "I am faithful to Wagner as there is something in all his music which is so majestic and so true to life." Ad says that he can hardly recall a single instance of obtaining a distinct emotion from music, he gets "a purely musical, intellectual pleasure, a unique mystic experience." The average musical level of the two groups is 4·4 and 7·5. But the majority, ranging over practically the whole scale of musical levels, fall into the intermediate class. Some of the fairly musical, for example Be, Dd and Ga, said that they obtained 'a state of mind,' 'a general impression' and 'an atmosphere' rather than specific pictures. Probably, apart from a few typical visualizers and a few highly trained musicians, the average listener indulges in short trains of association and lapses of attention, and while he frequently makes emotional interpretations, particularly of music to which words or a title are already appended, yet he also often listens technically and analytically, according to mood, familiarity, type of music, etc. He combines, more or less, all types of appreciation. Thus no hard and fast classification like Lee's can be upheld, though there is undoubtedly a tendency in this direction.

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[1] F. Howes, *op. cit.*

Finally, a slight correction in the Myers and Valentine classification is necessary as regards normal musical appreciation. The most typical visualizer seems to belong to a sub-class of the intrasubjective or emotional type as much as to the association or attention wandering type[1].

V. SUMMARY.

1. The musical appreciation of a large number of subjects was studied by means of questionnaires and specially devised concerts, where written qualitative and quantitative introspections were obtained.

2. Music has striking physiological, almost automatic, effects on the organism, stimulating bodily and mental activities. Wandering of attention and free trains of thought are often excited and may be highly pleasurable. The more musical, owing to a sentiment or musical conscience, tend to inhibit these primitive responses and to attend more actively to the music.

3. The resemblance of music to the sounds and motions of physical objects, especially when supplemented by suggestive titles or words, may [illegible]rouse a flux of concrete emotions on the basis of which listeners build [illegible]p elaborate dramatic interpretations or visual dramas, closely analogous [illegible] ordinary dreams. These are enjoyed for their own sake.

4. Though in most subjects lapses of attention may lead to trains of [illegible]gination, this imagery, which is directly stimulated by and which [illegible]ws closely the form, rhythm, etc. of the music, is comparatively rare. [illegible] incompatible with an objective attitude towards music, and is almost [illegible]lly absent among the most musical. Illustrations are given of these [illegible]ts, of the relation between visualization and emotion and fatigue, [illegible]d of the degree of agreement between the interpretations of different subjects.

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[1] For further discussion of the relation of Myers' character type to the intrasubjective and associative types, see P. E. Vernon, "Synaesthesia in music," *Psyche*, 1930, x.

(*Manuscript received* 10 *January*, 1930.)

AN APPARATUS FOR THE PHOTOGRAPHIC RECORDING OF EYE MOVEMENTS.

By M. D. VERNON.

(*From the Cambridge Psychological Laboratory.*)

THE apparatus described in this paper is based upon a modification of Dodge's method of recording eye movements photographically[1]. By means of the former, a record is obtained upon a continuously moving film of the movements of a bright spot of light reflected from the cornea. It has been shown by Dodge[2] that the movements of such a spot are the same in direction as those of the eyeball itself, but are only about half as great in extent. Fig. 1 shows the apparatus viewed from the front, and Fig. 2 shows it in section along a vertical plane, parallel to the sagittal plane, passing through the subject's right eye.

The source of light is a 100 C.P. Ediswan Pointolite bulb; the light is emitted from a small very bright ball, which forms an excellent point source. The light from the bulb D (Fig. 1), contained in the brass chamber C, passes through a blue gelatin filter and the narrow aperture, 3 mm. in diameter, of a diaphragm placed at J. The divergent beam of blue light passes along the brass tube T_1, impinges on the right-angled pris
P_1, and is totally reflected on to the cornea of the subject's right eye, a
shown in Fig. 2. The beam is reflected by the cornea in a direction parall
to the sagittal plane, at an angle of about 50° with the horizontal; a
impinges on a second right-angled prism P_2, by which it is refracted
totally reflected into the brass tube T_2 of the camera M. The diver
beam is focussed by the lens L, a Ross, London, Xpres camera le
aperture 1 : 4·5 and focal length 4⅝ in., and after passing through
tube T_2 and the aperture of the camera at A, forms an image on
film at F_1. In Fig. 2 the camera is shown in position for a vertica
moving film. The latter passes from the film spool F_2 over the two pulley
O_1 and O_2, and is attached to the heavy brass pulley O_3. The drive is conveyed from a motor and reducing gear to the fourth pulley O_4, the shaft of which engages with the shaft of O_2 by means of a worm gear. Thus the film travels smoothly downwards past the aperture A, and is wound on to the pulley O_3. In these experiments it was driven at a

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[1] Dodge, R. and Cline, T. S., *Psychol. Rev.* 1901, VIII, 145.

[2] Dodge, R., *Psychol. Rev. Monog.* 1907, VIII, No. 4.

1 j

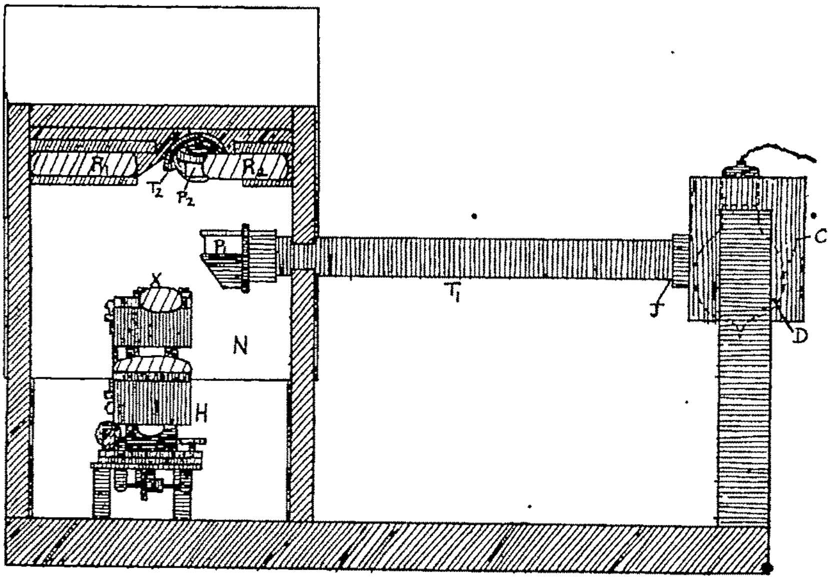

Fig I

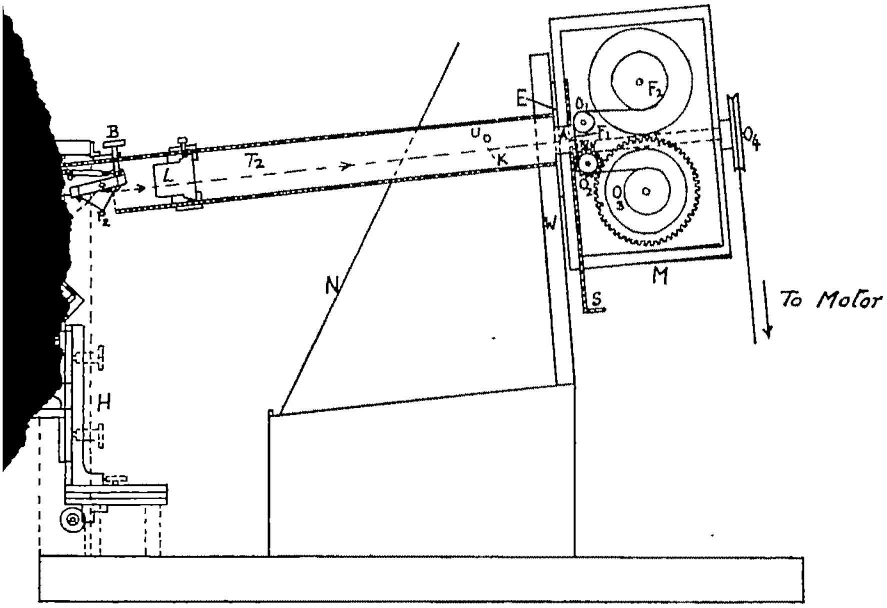

Fig II

speed of approximately 0·5 cm. per second. The aperture can be closed by means of a shutter, *S*, which slides in and out.

The experimental procedure is as follows. The subject places his head in position on the head-rest, *H*, with his chin on the chin-rest and his forehead pressed against the two adjustable pads R_1 and R_2. He grips with his teeth a piece of dentist's wax *X*; an impression of his teeth is taken, so that they can be held firmly in the same position throughout the experiments. Both the mouth and chin-rests can be adjusted vertically; and the head-rest can be moved as a whole in two horizontal directions at right angles to each other. By turning the tube T_1 about its axis, the beam of light is directed to form a bright spot of light on the cornea. The angle at which it falls on the cornea is such that, when the eye looks straight forwards, the beam does not enter the pupil, and is thus not visible to the subject. The reflected beam is directed along the tube T_2 by rotating the prism P_2 about its horizontal axis. The rotation is effected by raising or lowering the screw-head *B*; the prism is kept tightly pressed against the screw by means of the spring *G*. If necessary, the position of the head is also adjusted. The camera is previously removed from the upright *W*, and a piece of ground glass at the end of a tube placed in the position normally occupied by the film; the reflected image is then focussed on the ground glass by sliding the lens *L* towards or away from the camera. The latter is then replaced the motor started and the shutter opened. A photographic record thus obtained of the movements of the eyeball, made either volunta or in reading. The reading material, or a sheet of paper bearing a s of points to be fixated, is placed in front of the subject agains screen *N*.

The camera is held steadily in position by flanges fixed to a plate *E*, which in turn is screwed to the wooden upright *W*. If the sc are removed, the camera and plate can be swivelled about the tu until the camera and film are horizontal, and the metal plate screwed to *W*. With a vertically moving film, a record of the horizo components of the eye movements is obtained, and with a horizonta moving film, a record of the vertical components.

A time signal is used for measuring the exact speed of movement of the film. A narrow convergent beam of light is directed through the aperture *U* on to a small piece of plane glass *K* situated on the right-hand side (facing the subject) of the tube T_2. The light is reflected by this piece of glass to form an image at the right-hand edge of the vertically moving film. A time marker (not shown) placed outside T_2 in the path of the beam allows the light to flash once a second on to the film, thus giving a time record which does not interfere with the eye-movement record. This time signal can only be utilized with the vertically moving film.

This apparatus has been found to work quite satisfactorily, and a number of records have been obtained both of eye movements made voluntarily and of those made in normal reading. Fixations appear as fine black lines on the film. The speed of movement of the film is sufficient to show clearly the fixations and re-fixations which occur in normal reading. Voluntary movements up to 40° of arc can be recorded; they cover about 1 cm. on the film. By use of magnification, movements of 1° of arc and less are readily measurable.

In conclusion I wish to express my gratitude to Prof. Hartridge for his advice and help in designing this apparatus, and to Dr E. A. Schuster for constructing it at the National Institute of Medical Research, Hampstead; and to thank the Medical Research Council for permission to use the accompanying figures.

(*Manuscript received* 20 *December*, 1929.)

A NEW TYPE OF SPHYGMOGRAPH.

By W. A. WILLEMSE.

A. DIFFICULTIES OF EXISTING SPHYGMOGRAPHS.

ALL existing types of sphygmograph present considerable difficulties as follows:

(*a*) In sphygmographs with a wrist attachment the mode of transmission of the pulse-beat through a button held in position on the artery by a spring is inefficient. At the place of attachment the ordinary wrist is a bundle of tendons and bones, which often makes it difficult to get the lever button exactly on the artery and deep enough to register satisfactorily. Furthermore, the wrist is very pliable and responsive to slight finger movements, which may make the results registered difficult to interpret accurately.

It is, in fact, far better to make use of the brachial rather than the radial artery. On the inside of the elbow the brachial artery is very conveniently exposed. The muscles are larger, and do not cover the artery at all. The fore and upper arm can be conveniently fixed in the apparatus constructed for this purpose, and with the necessary precautions al [illegible] uncontrolled movements can be eliminated (see Fig. 1 *a* and *b*). That t [illegible] possibility of uncontrolled movement must be eliminated before t [illegible] sphygmograph is attached is a principle disregarded by all the ordi [illegible] types of sphygmograph.

(*b*) Sphygmographs like those of von Frey, Jaquet, Frank-Pete [illegible] Marey are not sufficiently sensitive. Every one of these is some [illegible] fication of the original principle of the Marey sphygmograph (F [illegible] in which a fairly strong spring is used to press the button on the ar [illegible] Much of the energy of the pulse drive is absorbed in overcomin [illegible] spring and is not available in order to displace a larger volume [illegible] for transmission purposes to the Marey tambour. This difficulty [illegible] some extent overcome in Lehmann's instrument, but this sphygmogra [illegible] cannot be properly fixed on the pulsating area. However, if modified in this respect—as in the type I suggest in this article—the Lehmann sphygmograph is very sensitive and is simple to work.

(*c*) Sphygmographs which do not make use of an air transmission can produce curves of about 1 cm. in height, but the chronographs attached to them make such a humming sound, and vibrate on the arm so perceptibly to the subject, that no accurate experiment is possible. Moreover, the spring which drives the mechanism cannot, without rewinding, go for a sufficiently long period to make even a moderately lengthy experiment possible. Also it is hardly possible to attach to these sphygmographs a time recorder, *e.g.* a Jaquet writer, and a stimulus recorder.

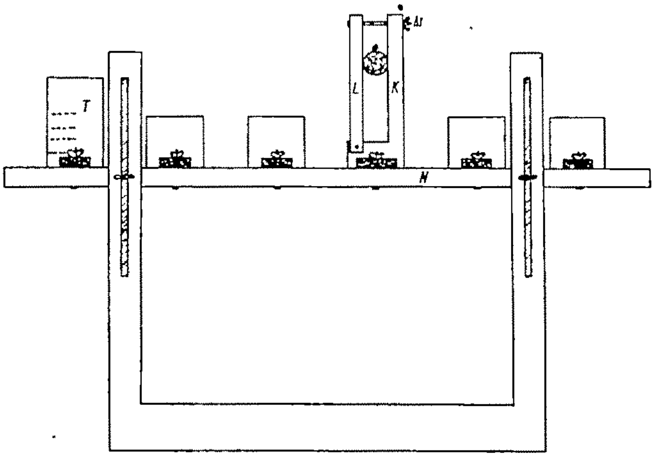

Fig. 1 *a*. Side elevation.

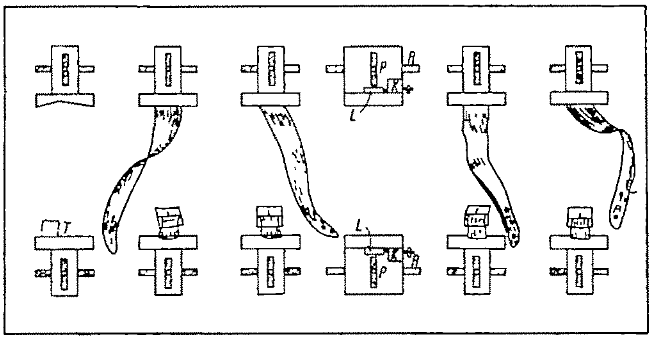

Fig. 1 *b*. Top elevation.

(*d*) The brachialis pulse-wave writer, though it escapes the pulse difficulties, does not seem to be applied to a sufficiently exposed small spot for sensitive recording of the pulse-beat. To get the Manschette firmly upon the deeply lying brachial artery such a pressure is necessary [illegible] the finer pulse changes are obliterated.

B. PROPOSED NEW FORM OF SPHYGMOGRAPH.

The type of sphygmograph I propose to some extent overcomes almost all these difficulties. In details it may still be much improved, and a very important central improvement would be to find a means of eliminating the air transmission which always to some degree entails a source of error. This could perhaps be done by having the sphygmograph fixed to a rigid frame, and adjusting the arm to the pelotte instead of adjusting the pelotte to the arm. Since in the sphygmograph in question the application is at the brachial (inside of the elbow), some such arrangement seems to be possible. In that case an ordinary kymograph, equipped with a Hering's extension, could be comfortably used.

The main idea of my modification of the usual sphygmograph will be quickly grasped by looking at Fig. 3. The air funnel *W* is covered with

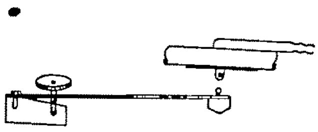

Fig. 2.

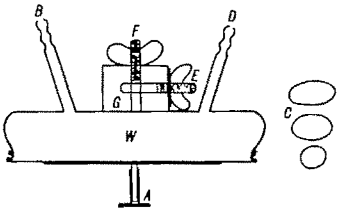

Fig. 3.

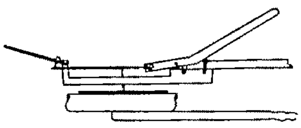

Fig. 4.

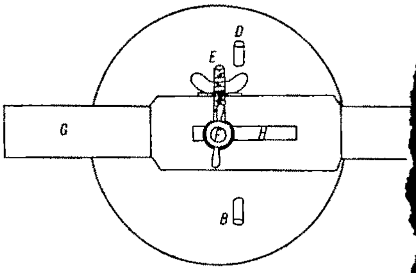

Fig. 5.

an india-rubber membrane to which a fairly wide aluminium cir
plate is attached. *A* is a detachable pelotte, the form of whose base ma
be varied (see *C*) to fit unusual types of fatness accumulation on the arm. To the one jet, *B*, a tube connected with a Marey tambour is attached. This Marey tambour (at the kymograph) is provided wit
a double lever to augment the size of the curve drawn
able force is necessary to move the double lev

CRITICAL NOTICE.

GESTALT PSYCHOLOGY AND GESTALT THEORY.

By O. A. OESER.

(*From the Psychological Laboratory, Cambridge.*)

FROM time to time the attempt is made to put a science or a philosophy on an entirely new basis, to set up all-embracing, all-explaining theories on which future research has to be based. Any such system must naturally concern itself with a critical examination of the foundations of that science or philosophy. It will have to justify itself by constructive criticism of existing systems, and will naturally come into conflict with existing ideas and prejudices. It must, therefore, expect to have its own postulates and deductions critically examined. Moreover, the sharper its own tone, the more intolerant of existing interpretations it is, the stronger will be the attacks levelled against it.

These remarks must serve as an excuse for the controversial tone of this paper, which will try to review some of the theories set forth in Dr Wolfgang Köhler's latest work[1]. It is claimed that nothing so fundamentally new has appeared since the time of William James and that [illegible] is book will revolutionize every aspect of Psychology. The whole tone [illegible] he book is extremely controversial or dogmatic; certain statements [illegible] nade over and over again, as though mere reiteration could *prove* [illegible] ing. It has not been an easy task to sift and select; but wherever [illegible] necessary, we have quoted Köhler's own words to prevent mis[illegible] tanding as far as possible.

[illegible] e all other philosophies and most psychologies, *Gestalt* psychology [illegible] what is the nature of the higher mental processes and what part they play among the phenomena of the universe? The answer to this question is a complete inversion of all idealistic systems and an attempt at a thoroughgoing materialism. What we call 'mental' structures, are supposed to be entirely based on the physical structure of nervous processes. Ideas rest on physical reality and not on any platonic '*eidos*' [illegible] r *a priori* mental categories. This is thought to be possible because within the physical world we find structures to be operative that are in [illegible] respects similar to the structures of the higher mental life, namely, physical *Gestalten*. These physical *Gestalten* are the ultimate foundations of mental life; and the formal structure of mental life is already present in physical processes. These foundations can therefore not be arrived at by the method of introspection, since the attitude of introspection changes an experience during and by the process of introspection; nor by the purely arbitrary and external methods that behaviourism imitates from physics, since these are based on a mistaken idea as to the functional validity of atomistic mechanical laws. *Gestalt* psychology shows that such laws do not exist for the mind: it thinks that the *real* laws are known to us immediately in 'direct experience' and that we need only the naïve acceptance of 'direct experience,' and no other principle than that of 'dynamical self-distribution,' in order to explain the structure of the mind[1].

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[1] Dr Wolfgang Köhler, *Gestalt Psychology*, G. Bell & Sons, London, 1930. The page [illegible] nces in this notice refer to the original American edition.

This applies to every form of psychological experience: "According to the theory of dynamical self-distribution color as a quality is dependent upon chemical reactions in a brain-field....There is much more that is characteristically spatial and dynamically real in such a coherent distribution than in the mosaic of local sensation-processes which are the 'dynamical realities' of machine theory[2]." Why do we experience 'constancy of brightness'? What is an attitude, an instinct, attention, admiration, the quality of 'softness' or 'roundness' in a voice and why do we experience them? Because they are based on dynamic tensions and stresses in dynamic physiological *Gestalten*, because, or just a potential difference, charge distribution, surface tension, orbital celeration, are physical *Gestalten*.

What is a *Gestalt* and whence does this word derive its potency? It seem largely a case of word-magic. Let us take some of Köhler's definitions: "In the word 'Gestalt' may be used as a synonym for 'form,' or perhaps 'sh von Ehrenfels, taking the case of specific shape as the most important and among his qualities, applied the name of 'Gestaltqualitaten' to all of them. Th it will be clear that not only the different forms or shapes of objects and figure included, but also qualities like 'regular'[3]." Spatial and temporal 'Gestaltqualitäte must also be included. But since the time of Goethe *Gestalt* connotes shape and form as a *property* of things, as well as "a concrete individual and characteristic entity, existing as something detached and *having* a shape or form as one of its attributes[4]." But it is chiefly in German literature of to-day that the word *Gestalt* figures and is clothed with a mystical meaning. As it is used to-day, the word means the *ide* configuration, which shines through its earthly manifestations. It is important t remember this, for it seems to have led large groups of people into believing that th aspect of the word had at last found an incontrovertible scientific basis in the pr

[1] See below, pp. 76 ff.

[2] P. 190.

[3] P. 191.

[4] P. 192.

that there are physical *Gestalten*. Since the theory clothed itself in a veritable wilderness of differential equations, and never ceased emphasising that *Gestalt* was the solution of every fundamental problem in physics or psychology, it was accepted without further comment by those incompetent to judge, or ignorant of physics, and hailed as a revolutionary theory[1]. But let us return to Köhler. "Gestalt means any segregated whole." "The same general type of dynamical process which leads to the formation and segregation of extended wholes *will also explain their specific properties*.... This, indeed, is the most general concept of *Gestalttheorie*: wherever a process dynamically distributes and regulates itself, determined by the actual situation in a whole field, this process is said to follow principles of *Gestalttheorie*.... In conse- ence the concept of *Gestalt* may be applied far beyond the limits of sensory fields[2]." n general, "*Gestalten* are states and processes whose specific properties and effects cannot be compounded from properties and effects of a like order shown by their so-called parts[3]."

These definitions are so wide that they tell us nothing. A *Gestalt* is any segregated whole; it is also a state or a process; it is more than the sum of its parts. A chair is a whole; it is more than the sum of its parts, since I cannot sit on any of its parts. Is a chair a *Gestalt*? However, for reasons of word-magic we must retain this word, the more so since Köhler believes that it also stands for a new concept in physics.

We shall have occasion to return to these definitions later. Let us first follow Köhler in his exposition of the principles that underlie psychological events and the reasons for believing them to be in all respects similar to certain events in the physical world. We need not concern ourselves very much with the first chapters, which deal with the viewpoints of Behaviourism and Introspectionism. He presents these heories in an extraordinarily narrow manner. No psychologist of any nding has ever held them in this form. He frequently digs up old rines that have long since been discarded. Naturally, it is easy h to smash up any theory, if only one presents it in a sufficiently ed fashion. If Introspectionism really is the simple and relatively theory it is made out to be, we should have thought it unnecessary öhler to have taken such pains to demolish it.

nstead of using the word 'consciousness,' which to some people eans a function by or in which we become aware of 'immediate experience,' Köhler proposes to use 'direct experience' throughout, since, if someone has a 'feeling of becoming aware,' that is merely one special case of direct experience. For a child—or a layman, whose judgment on many fundamental aspects of psychology is far more to be trusted than that of trained introspectionists of philosophers (cf. chapter on 'Insight')—objective experience is not something "felt by me as being 'given in my perception' and therefore as 'subjective'." Things and events are *simply there outside*. Anything more objective is inconceivable to the child, since they are independent of its presence, or of its keeping its eyes open. Therefore, in this book, when Köhler speaks about 'direct experience of the objective type' he means "e.g. a chair as something there outside, hard, stable, brown, generally without any taint of 'being perceived' by me, of being 'a subjective phenomenon'[1]." Now for th behaviourist there is no such thing as direct experience (*i.e.* consciousness); for the introspectionist direct experience changes in the process of being observed. Furthermore, physical measurements are indirect, and the physical world, that is built up by inference and is known only mediately is, in a sense, subjective. No two physicists can ever observe 'the same galvanometer,' since each only has his direct experience to go upon. But, Köhler argues, extreme epistemological purism shows that the existence of the physical world can never be proved conclusively, nor the existence of direct experience in others. "What is needed, then, is a little less epistemological purism and a little more *naïve* pragmatism." The starting-point for psychology must be that 'direct experience of the objective type' which the child has. The necessity for this becomes even more cogent when we remember that every science started off with purely qualitative observations and that psychology, being only in its infancy, cannot imitate the most advanced methods of physics physiology, being, for one thing, unable to create its own systems regulate their controlling factors. Psychology should at this stage co itself above all to qualitative observations.

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[1] Cf. the critique by Jaensch and Grünhut, *Über Gestaltpsychologie und Gestalttheorie*, ngensalza, 1929.

[2] P. 193. Author's italics.

[3] W. Kohler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, p. xiv, aunschweig, 1920.

But instead of adhering to this principle, and giving qualitative observations, Köhler goes on to suggest a new bas building up a working hypothesis and formulates the following si principles[2]:

(1) *Naïve description.*

We must always utilize the direct experience of the subject and accept it at its face value, since it will always give us hints about the psychological processes we are seeking.

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[1] Pp. 21–22. The sentence is presumably meant to read: "a chair [experienced] etc." Direct experience is thus simple awareness, untainted by introspection, meaning experience, or reason.

[2] Kohler does not classify or name them as here given. That has been done by th author to make them easier to refer to.

(2) *First primary psychological postulate.*

"There is no doubt that this experience depends upon some of those processes about which we wish to formulate a working hypothesis. It is more than probable that, under these circumstances, I may use my direct experience in order to guess about these processes[1]."

(3) *The psychological theorem.*

"There is no reason at all why the construction of physiological ocesses directly underlying experience should be impossible, if experie allows us the construction of a physical world outside, which is lated to it much less intimately[2]."

Thus, if the layman feels 'admiration' for someone or something, he also feels this as a 'directed tendency' in direct experience. Therefore, not only is there a physiological process underlying the 'admiration,' but in it, or between it and the process underlying the self, there will be a dynamical stress, or a vector (cf. chapter on Insight). The most astounding part of this theorem is that it should be just as possible to construct this physiological world on which our experience is based, as it is to construct the physical world—which we know only through the mediation of physiological processes! Is there one of these dynamical physiological processes that we can isolate and study? Only 'direct experience' tells us about them, and to everyone except Köhler this is a very uncertain witness. The physicist has long ago discarded it in its naïve form. He does not assume that a dish of lukewarm water is hot because his hand, which has just been immersed in ice, gives him the experience of hotness[3].

4) *Second primary psychophysical postulate.*

In order to guarantee the correctness of my inferences from direct erience to the underlying physiological processes I must assume *ence or isomorphism in their systematic properties*[4].

rst secondary postulate.

physiological processes are *dynamical* units.

ond secondary postulate.

e physiological processes corresponding to different parts of an rienced visual field in which there are a number of configurations kept from intermingling by '*separating forces of contact*' (see below, . 82).

(7) *Inferences from* (1)—(5).

(*a*) *Order.* We are led at once to "a bold hypothesis which has -reaching consequences, if it is taken seriously enough." If we find in direct experience that one point, whether spatial or temporal, lies between two others, this experience must be accompanied by "a functional 'between' in the concrete dynamical context of concurrent physiological events," simply because it is an immediate experience. I do not reason about the matter: the 'betweenness' is itself an experience and must necessarily have its corresponding physiological process. Hence "*experienced order in space (or time) is a true representation of th corresponding concrete order in the underlying dynamical context*[1]."

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[1] Pp. 59–60.

[2] Pp. 60–61. This passage is italicized throughout.

[3] See the discussion of physical principles below, and particularly the summary at the l of this paper, pp. 93 f.

[4] P. 61.

(*b*) *Language as a symbol.* I write down or speak about my obser tions, and the language is taken by me to refer to my own direct expe ence, which has a concurrent physiological process. "Therefore, if, to m my language is an adequate symbol for my own direct experience, it is an objective symbol for those physiological processes at the same time[2]."

(*c*) *Context.* If I sit in a concert hall, I hear several singers, my neighbour's snores, the shuffling of feet and numerous other sounds. I also have a large number of visual experiences. Nevertheless, they are not all inextricably mixed; the sounds coming from the singers are experienced as 'belonging together,' those from the accompanying orchestra 'belong together,' and so on. This 'togetherness,' the *context*, in this example as in that of a sentence or any other case, is a direct experience. Therefore, "*To a context, experienced as 'one thing' belonging together, there corresponds a dynamical unit or whole in the underlying physiological process*[2]."

(8) *First axiom of originality.*

"This last application of the principle (7 *c*) has the greatest i ance for *Gestalt* psychology. In this form, at least, it becomes a physiological hypothesis about common sensory experiences *as about the most subtle and complicated processes producing ob behavior*[3]."

(9) *Second axiom of originality.*

Dynamics itself *produces* the order found in physiological events[4]

Instead of 'a little more naïve pragmatism' Köhler has given us most formidable list of assumptions, most of which seem to assume i advance what he is really setting out to prove. And yet, how simple it all sounds. I see a figure composed of five dots, ⁚•⁚, as a whole: the underlying physiological process must be a unit. I have a direct experience of joy, feel anger, hear a symphony, admire a singer, speak a sentence, experience the sum of my spoken sentences as a good speech, see that one tree stands between two others, feel charmed on perceiving the first smile of my child[1]: behind all these experiences there are physiological dynamical units, or dynamical stresses. What sort of dynamics? Where do these dynamical processes take place? What is it that is dynamically interacting? But we must not ask questions like that, because, "Confident as we are of further help from physiology, we are so utterly in the dark at present that it would be pure folly not to profit by any light that may fall upon some cases at least.[2]" And at any rate we shall obtain some light from a study of physical dynamic systems.

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[1] Pp. 64–65. Kohler's italics.

[2] Pp. 66–67. Kohler's italics.

[3] P. 66. Author's italics.

[4] If it is not an axiom, it is mere tautology for the law of action and reaction physics, which is discussed later. It is original, because no one ever thought of dynam having this function before.

So far, we do not seem to have got anywhere in particular. We have, it is true, some "bold and radical physiological hypotheses with far-reaching consequences." The hypothesis that to every experience whatsoever there corresponds some physiological process is not new. But the *formal* structure of experience, its systematic properties, are also supposed to be found in these processes. Since in 'direct experience' no such thing can be found as 'a *mosaic* of indifferent stimuli,' therefore any such physiological process is a *dynamical unit*. Since my experiences show a certain spatial or temporal order, this order must be due to a self-regulating property inherent in all dynamical processes, and cannot be due to mechanical arrangement. Out of the material processes spring ra material factors and it is these that make experience possible. does this come about? We must study the world of physics to get wer.

find in nature, Köhler says, two factors: constant topographical s and dynamical *processes*. The constant conditions are the f the dynamical processes, since the more 'constant' factors we ce, the more restricted is the play of dynamical forces. Thus, in internal arrangement of an atom the dynamical factors are allowed greatest latitude, less in the distribution of charges over a conductor, still less in the piston of a machine, where only *one* direction is possible. Now—and this is the second axiom of originality (cf. p. 78)—it has always been thought that the only *order* that is ever to be found in nature is that which is enforced by topographical conditions (as in industrial machines). If dynamical forces are allowed free play, only chaos would be produced. But this view is quite mistaken[3]. If we study the atom, electrically dissociated solutions, or a drop of oil in water, or condensers, or any other physical system, we see there a beautifully harmonic, orderly distribution, which is brought about by the free dynamical interaction of various forces. "Everywhere in nature dynamical events depend upon the properties of those processes and materials which exert influences upon one another[1]." But hitherto it has been assumed that only machines were orderly, and nerve currents were therefore thought to be conducted through definite 'wires' to definite spots in the brain, and that the properties of the local processes thus excited would have no influence on each other.

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[1] P. 353.

[2] P. 60.

[3] That is why it is *not* held in physics!

In passing we may draw attention to the fact that such an extreme 'machine theory' has never been seriously held in modern experimental psychology, at any rate since the time of Ewald Hering. It used to be considered that to every stimulus there corresponded a certain response, just as every time a key is struck on the piano a definite string vibrates. Since it was very soon found (e.g. by Helmholtz in his work on vision) that this was hardly ever the case, it was assumed that at any rate genetically this state of affairs must have been the primary one and that only in later stages of evolution, meaning and accumulated experience added something to these 'primary sensations.' But Hering and his school insisted on the 'autonomy of organic processes.' They drew attention to the fact that to one stimulus there could correspond whole ranges of sensational contents and that therefore psychological observations should start, not from an analysis of *physical* manifestations of matter (wave-lengths of light), but from sense experience itself. Katz, Jaensch and many others investigated the way in which the sensations of light depended on their spatial context. In connection with the phenomena of the 'constancy of brightness,' the approximate 'constancy of colour' and in the field of space perception, Jaensch an[illegible] his pupils in particular were able to show that there is no such thing as a 'prim[illegible] sensation.' On the contrary, the primary fact is the perception of 'things.' Helm[illegible] in the chapters on spatial perception of his *Physiological Optics*, also characteri[illegible] perception of objects (i.e. 'wholes') as the factor which predominantly de[illegible] perceptual processes. Indeed, if this were not so, we could not understan[illegible] object could be an 'invariant' for us. A piece of paper in the dim light [illegible] would look like coal—since photometrically coal in sunlight emits as muc[illegible] unit area as paper in a deep shadow—and every fleeting shadow or change in [illegible]tion would change the paper. That in the case of perceptions wholes are more [illegible] their parts, that, indeed, the parts are often determined by their whole context, is [illegible] fundamental biological significance. But the way in which they so depend cannot be explained from purely physical principles. "If dynamics are excluded from the determination of distribution, local process will correspond in every case to its stimulus; the actual properties of stimulation in their mutual relationships will play no rôle in the whole affair, as they would undoubtedly in the case of dynamical interaction[2]." This conclusion is drawn by Kohler first, because he has this curious 'static' idea of machine conduction, secondly because he thinks that order can only be achieved in the case of the type of 'whole' with which sensory experience de[illegible] if there is 'dynamical interaction.'

[1] P. 120.

[2] P. 127.

Köhler gives numerous examples of cases in which observation has proved that properties of local sensory stimulation cannot be responsible for the actual perceived facts or their internal order, such as seen movement (the stroboscopic experiments of Wertheimer and others), colour contrast, the Müller-Lyer illusions, tonal fusion, etc. The machine theory—"a policy of least scientific effort"—is unable to cope with them; the only solution is 'dynamical interaction,' which can, for instance, take place in the gray ganglionic and nuclear fields of the brain. Is this any vital advance? We are still dealing with purely physical mechanisms. Or does Köhler want to make us believe that dynamical distribution is not a physical mechanism? Indeed, no: for he quotes "a few out of the millions of examples" of such interaction and distribution—drops of water, charges, ions, etc., in support of his contention that physical systems also have *Gestalt* properties.

That is one of the characteristics of this book. A certain number of phrases are repeated over and over again, with tremendous emphasis, wherever more detailed explanation is called for. The result in the reader is eventually a conditioned reaction or a hypnotic trance: if the explanation of every phenomenon is "the dynamical distribution of process-in-extension," one eventually begins to believe it; sheer reiteration batters one into acquiescence. But what we are to make of any of the statements is not quite clear; a drop of water in a pipe can ly move in one direction under pressure. But a drop in a large volume es at each point along the resultant vector of forces, and 'dynamical ution' takes place. "In pipes, order is produced by *exclusion* of cal interaction; whatever distribution may result in the 'dy- ase' is *produced* by dynamics itself[1]." What is the difference in ses? In the pipe, the pipe itself prevents the water from spread- ut each molecule or drop moves along the resultant vector of rces from other molecules just as much as it does in the case op in a basin, or ocean of water, where the surface of the large s prevents the drop or molecule from leaving. In both cases there is exactly the same amount of dynamics. Each movement of any drop of water is rigidly determined, theoretically calculable and predictable in either case. All Köhler's examples are no more than tedious illustrations of the oldest law in physics: that of action and reaction. However, ents of this (dynamical) type "are excluded almost completely from hines, and the same type of process is practically excluded by ndard neurological and psychological theory. *Gestalt psychology asks*

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[1] P. 135. Kohler's italics.

to know the ground for its exclusion and defiantly proposes to give this type of process a fundamental rôle in psychological theory[1].”

At this point Köhler states categorically that both to Aristotelians and to modern theorists dynamical interaction is a synonym for disorder, since *anything* may happen! It is beyond us to imagine any physicist making such a statement. Nor did Aristotle make it. To him there were, *in addition to* the mutual interaction of parts in any physical process, special ‘tendencies’ that brought about any particular state. This was also the physics of pre-Galilean times. Since Galileo the conception of a special ‘tendency’ for each event has been discarded, for obvious reasons. But Kohler introduces it again as ‘new,’ as we shall see.

Having now settled once and for all that the ‘processes in the field’ are dynamic, we must explain why the figures of the tablecloth and carpet before me remain separate, why they do not fuse into one vast dynamical organization, to give an orderly distribution of all the parts. We sometimes do get fusion, it is true, as in diffuse contours, or after the tenth whiskey. But on the whole perceived objects are clearly separated. How is that? The explanation, according to Köhler, is very simple. Take the case of oil in water. The interaction of molecular forces is so powerful that the surfaces become separated; in the case of alcohol and water they fuse. “I shall assume then, that, in optical processes, contours are preserved by similar *forces of antagonistic contact*, depending upon differences in the properties on the two sides of the contour.” “Generally
processes corresponding to a definitely colored area will have definit
properties as a class of processes, different from the properties of a s
rounding class of processes which correspond to another color. They wi
main isolated in the nervous network if we suppose that in the gan
fields, where they ‘touch’ each other, their differential propertie
separating forces of contact, so that they mutually exclude each

We obviously cannot assume that, if I see a hundred different pat
carpet with thousands of different separating lines, there are just so ma
of these ‘separating forces of contact.’ We must assume either that
many thousands of forces; or that the few that are operative are static an
like wax; or that there would be a tendency to see in circles. According to
the last is actually the case. When an after-image dies down, it takes on a symmet
form, and memory images of complicated figures get ‘smoothed down’ in the process of time. But what about my memory images of playing tennis? Later on we find more *ad hoc* hypotheses of the same type: boundaries are places “where forces of ‘segregation’ and ‘separation’ take the place of those of ‘coherence’ operating elsewhere[3].”

There is one further point of importance. In all these ‘dynam
distributions’ among drops of water, etc., we know that the acting fo

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[1] P. 135. Author’s italics. [2] P. 143. [3] P. 148.

are constant. If they are not, *e.g.* if the water is stirred irregularly, there will be no dynamical equilibrium. That is why Köhler emphasizes that order is only produced in *undisturbed* dynamical interaction. But in organic systems we would seem to have precisely this irregular production of forces. Therefore we must either assume that 'dynamical processes-in-the-field' are not affected by them—and we obviously cannot make that assumption; or we may apply the principle of 'separating forces of contact'; or we can ignore this particular point and say "in organic systems all inertial velocities will be destroyed by friction," so that these systems will necessarily provide us with perfect examples of the distribution by dynamics leading to rest or stationary processes[1].

Köhler insists that segregation of wholes in the sensory field is a primary fact. Such wholes only afterwards acquire meaning. Indeed, *Gestalt* psychology "even goes so far as to hold that it is precisely the original organization and segregation of circumscribed wholes which make it possible for the sensory world to appear so utterly imbued with meaning to the adult, because, in its gradual entrance into the sensory field, meaning follows the lines drawn by natural organization[2]." Thus a person walking along in a mist may see certain segregated wholes without in the least knowing what they are. They may remain mere
'patches' until they begin to mean something, when their whole organ-
zation may become completely altered. But we must not attach any
al importance to the influence of meaning. It is argued, for instance,
t all the groups we know consist of physical objects, which are per-
d as moving together. Therefore, whenever a group is seen, this is
be some physical object, *i.e.* it has a meaning attached to it at
ut this is quite fallacious; for five flies sitting on a wall may be
d as a perfectly definite group, although next minute they all
awl in different directions. Again we ask, has this example
ything to do with the former argument? That concerned itself
more or less homogeneous groups, not with singular points. If one
w a continuous patch of the same size and boundary configuration as
the group of flies, one would most certainly be surprised to find it moving
in all directions, and would immediately take it to *mean* that it was a
jelly, or a mass of minute black animals.

This again illustrates the difficulties behind the theory of 'separating
es of contact.' Consider the two figures on p. 84: one certainly experi-
s two 'groups' of dots. Therefore the underlying dynamical context
mposed of two units or wholes. But each group has within it a further

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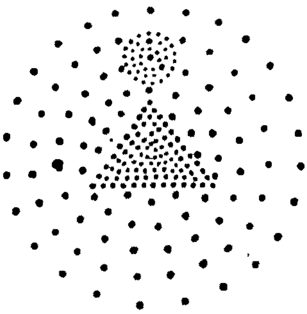
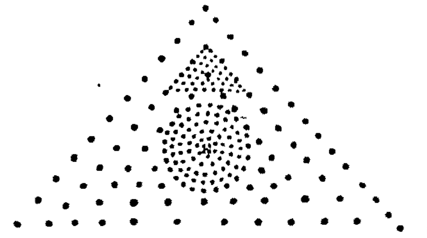
[1] P. 139.

[2] P. 152.

group, as well as a large number of isolated points. How does each point become differentiated within the dynamical unit by these separating forces? And why should we have separating forces here, but not if the interior of the two figures is entirely blacked out by microscopic points, whose distance apart is just too small to be separated by unaided vision? It is obvious that at any rate "the relations of neighbourhood, of similarity and difference among the stimuli, though these are indifferent to each other dynamically, are in some respects a copy of the corresponding relations among the surface-elements of surrounding objects. Some definite relations among the stimuli determining sensory organization, and the preservation of these relations in transmission, seem to be the essential conditions for that reconstruction of objective 'belonging together'[1]." "The equality or similarity [of particles in a group] a their common difference from other 'particles' favor their becom grouped together and their segregation from others[2]." The organiz of the field is not independent of the *relative* properties of stimul it is dependent on relations that are inevitable in a physical Just as groups of dots may be 'organized,' so taps may be ' into groups of temporal 'dots.' "*These groups represent an* *physiological, or, if one likes, psychological organization*[3]."





Although the formation of groups, while being an elementary nevertheless depends on certain relations of similarity and difference, t relations of these groups among each other, as well as any other psychological experience, depend on 'tendencies,' or on 'stress.' Thus "a change of attitude always involves a definite physiological stress exerted upon a sensory field by processes originating in other parts of the nervo system, and to some degree the organization of the field may yield it[4]." Why is our attitude of dislike taken up towards some objects situations? Why do I move away from a radiator that is too hot? Because "there is a field of force tending to increase the distance between the place of the radiator and the place of the self[1]." When I begin to move away, that movement is experienced, and gives rise to a new physiological dynamical distribution, which is felt as being in the direction of the former stress. "Therefore, physiologically as well as in experience, what happens will be no more than the natural consequence, the real development, or *the evolution of something which, in germ, was already implicitly contained in the tendency*[2]." And this is no more and no less than *insight*[1]. In exactly the same way we must interpret instinct and reflex actions. The organism moves in the direction of the physiological stresses set up between various processes underlying various relevant experiences, and the instinctive action is no more than the outcome of the tendency of these stresses to come to a state of equilibrated dynamical distribution.

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[1] P. 182.

[2] P. 155.

[3] P. 164.

[4] P. 184.

It is unnecessary to pile up further examples of the uses to which the fundamental 'radical physiological hypothesis' is put. Every 'direct experience' is the result of dynamical processes in the brain-field; action, insight, association, reproduction, observable behaviour, are all the outcome of dynamical tendencies in these processes themselves, or in their traces, which are probably some form of molecular concentration. We may admit that we can see no adequate reason why behaviour and production should not be based upon something more complicated a mere impress of disparate stimuli having no sort of relation to other (except the inevitable ones of neighbourhood, similarity, etc.). y agree with McCurdy that it has something to do with 'patterns,' Köhler that it is bound up with *Gestalten*. We shall also be o agree with McCurdy that, whatever we may say about the rganization of such patterns, the way in which they actually bout, say, reflex activity, and their nature still remain "the amental mystery[3]." But Köhler will have none of this. If McCurdy patterns are 'a fundamental mystery,' it is quite obvious that he knows nothing about 'physical *Gestalten*[4]' as these have been elucidated by Köhler. What, then, are those properties of physical *Gestalten* that are the same as mental structures, or that are the root and final explanation all experience?

P. 390.

P. 391. Author's italics. Insight is thus thought to be no more than a mechanical

John T. McCurdy, *Common Principles in Physiology and Psychology*.

[4] W. Köhler, in his review of McCurdy's book in *Die Naturwissenschaften*, Heft 20, 1929.

These properties are very simple; so simple that it is extraordinary why physicists have not tumbled to them before. It would have saved psychologists a lot of trouble, for they need not have tried to base psychology on the mere senseless interaction of completely unrelated elements. "The dawn of the *Gestalt* problem in modern psychology was not the idea of dynamical self-distribution as opposed to order enforced by arrangement;...the starting-point was the observation that sensory fields are replete with qualities and properties which one neglects if one takes 'sensations' as their sole content and which, indeed, may have a mysterious aspect when first viewed in this way. It was von Ehrenfels who...directed the attention of psychologists toward the fact that a great many, and perhaps the most important, properties of sensory fields do not fit into the scheme of concepts which is centred around the idea of 'sensation'[1]."

The two properties which sensory wholes possess are best exemplified by a melody. If the different notes that make up a melody are presented separately to different people, the sum of their experiences will not be the same as the experience of one person to whom the melody is presented as a whole. To him it has something more than the sum of its parts, its specific melodic form. Furthermore, the melody will still be recognized if it is transposed to another key, that is, if every vibration is altered in
a certain way. Chickens can be trained to respond to a certain shade o
gray, which is the darker of two shades presented. If both these gra
are changed, the chicken will respond to the gray which is the dark
this second setting. Their response does not depend on the ab
intensities of the stimuli at all; it depends on the whole situation
relative properties of the stimuli. These, then, are the two fun
properties of *Gestalten*: (*a*) the whole is more than the sum of
(*b*) *Gestalten* (or wholes) can be transposed.

Now the fundamental psychophysical postulates (cf. p. 76)
all experience is based immediately on physical processes, whic
dynamical units. But a physiological process is an event in the mater
universe. Hence, "in order to prove that dynamical self-distribution will explain 'transposition,' we have to show that 'transposition' is possible in self-balanced physical systems. Nothing can be easier![2]"

Consider an electrolytic system containing Na and Cl ions. T
charges will be carried equally well, and the 'self-distribution of
current' will remain the same, if we substitute Br and K ions. Again
potential difference between two solutions is unaltered if the con

[1] P. 187. [2] P. 218.

trations of each are altered in the same proportion. The solution that was the electropositive side when the concentrations were $\frac{1}{20}n : \frac{1}{4}n$ will still be the electropositive side when the concentrations are $\frac{1}{100}n : \frac{1}{20}n$. "To be 'the electropositive side' of such a physical system is no less a *Gestalt property* in a definite electrochemical whole than to be 'the dark side' is a *Gestalt property* in a sensory pair[1]." In physical systems, therefore, we find precisely the same Gestalt properties as we find in a melody: if two solutions are kept separate, there will be no potential difference, nor has either solution alone a potential—*i.e.* the whole is more than the sum of its parts. Also the system can be transposed without altering this very characteristic, which appears only when the system is considered as a dynamic whole.

How is it, we may ask, that physicists have not discovered this before, so that it needed a psychologist to point out the existence of *physical Gestalten*? The answer is, first, that both von Ehrenfels' criteria mean one and the same thing; secondly, that physicists do, in fact, study nothing else, except that they call the physical *Gestalten* physical *systems*. This term has been in use for so long that there is no sense in changing it, less sense still in mixing it up with Aristotelian, pre-Galilean 'tendencies' or mystical conceptions about the potency of an '*eidos*.'

Köhler considers the first criterion necessary but not sufficient, the cond sufficient but not necessary. Hence both must be applied toher. Actually, however, they are synonymous, and not at all adequate. can be no two or more things without there being relations between They will have relative geometrical positions. Since they form a , the laws of mathematics, in particular the theory of groups, to them. Thus they will belong, say, to the group of countable bers and will share its properties *over and above* the mere fact thmetical sum. That gives us our first criterion. But as soon condition is fulfilled, the group is transposable, for the theory numbers does not deal with stones or cows, but with 'indefinables.' Similarly, an ellipse is more than a mere sum of geometrical points. It is also transposable into other co-ordinate systems, where its fundamental roperties remain the same. To take a concrete example: any group of ns in England stand in a certain relation to each other, which is than the sum of the parts. For, if Edinburgh were transported to , it would no longer be 'north of' London, although the mere sum e two towns has remained the same. But the relations between the towns remain the same when transposed to any number of maps of different scales. It is because relations do not depend on absolute values that both von Ehrenfels' criteria mean the same thing. And it is spatial and temporal relations that are measured by physics, before it begins to study causal relations. Indeed, the possibility of formulating a general law depends on the fact of 'transposableness.' The gas laws apply to O_2, Cl_2, N_2, and any other gases. Köhler cannot show us any physical system in which these criteria do not hold. But they are purely descriptive and tell us nothing more than: if there are parts, there will be something more as well, which vanishes when the parts are separated, and which remains when their relationships are altered proportionally. They are, in fact, simply another way of expressing the law of proportion; but they do not tell us whether it is the whole that determines the 'more,' or the parts, or neither.

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[1] P. 219.

There must be another reason for expecting so much from physical *Gestalten*. It is to be found in Köhler's repeated contention that the Gestalt is *produced* by the dynamics itself; that a dynamical distribution, as of current in our electrolyte, is something fundamentally different to a distribution enforced by topographical arrangement.

Moreover, we have the "superstition which is thousands of years old" that dynamical processes when left to themselves only produce chaos. Physics, Köhler thinks, explains physical processes purely mechanically, a in the kinetic theory of gases, by the motion of small particles; the di tribution is one of chance. But no one ever uses the word 'mechani in this sense, that natural processes are summative combinatio mutually independent parts. A machine is a mechanism: b *function* of every part is determined by that of every other. A oil comes to rest in water. Is there a process connected wit that goes on independently of the processes in the water? that somewhere in a factory HNO_3 were produced out of i and that in another part of the factory the product of that c organization were used to dissolve silver—would you say that the si reacts to Nitrogen, Hydrogen and Oxygen? You certainly would not, because what happens to the silver depends upon that chemical organization, and it cannot be understood as a reaction either to those element separately, or to the sum of them[1]."

If we look into these physical systems a little more closely, wha we find? In the case of dilute electrolytic solutions we need to only two things: the ionic mobility, that is, the velocity of the which is found experimentally, and the laws of the kinetic theory of gases. When we know these, the potential difference that arises when two solutions are in contact can be explained purely *mechanically.* It can be calculated from the known ionic mobilities and the gas laws for any number of such systems in contact.

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[1] P. 180.

Again, no physicist believes that he cannot understand the reaction of silver for HNO_3 from his knowledge of those elements. He has studied the structures of the four atoms concerned; he knows, or hopes to know soon, what the resultant structure and properties of the HNO_3 molecule must necessarily be; from this and the known structure of the silver atom, the reaction follows necessarily, again on the basis of 'mechanical' laws. There is no 'tendency' in the HNO_3 and Ag to attract each other. It all follows from the laws of electricity. In the last resort, of course, these are based on the peculiar properties of positive and negative charges; and here we may speak of 'tendencies,' since we cannot as yet fathom all the depths of the universe. All of physics is perhaps built up on this *one* 'tendency,' not on millions of *ad hoc* 'tendencies.'

But this is not at all what we understand by 'tendency' in organic systems. What we mean when we say a process is 'mechanical' is that it is determined completely at every instant by x factors (charge, concentration, temperature, etc.), which do not *of themselves* belong to-
gether, through some inner necessity. These factors coming together,
e law of action and reaction determines the entire process. If another
or is added, the process may turn out quite differently, while still
ning strictly predictable. That, indeed, is how the scientist works.
ways seeking out those x factors. When a process with which he
r suddenly takes another course, he does not imagine that it
red with the old ways and wants a change; he infers that a
has entered, which is contributing its quota of action and
nd sets himself to discover it. The relative causal inter-
nce of factors is therefore the fundamental presupposition of
ural 'mechanism.' A chemical process is called 'mechanical' because
none of the x factors belong together through inner necessity; never-
theless, once they are together, a variation in any one factor necessarily
hanges the process as a whole. We do not assume that two solutions
me together because they want to produce a potential difference,
h neither can produce by itself. If they happen to come together
ugh the intervention of the chemist), this potential difference is
uced inevitably by mechanical laws, whose operation is fully deter-
ed and foreseeable at every instant. There is no "classification of

physical systems which is decisive for our problem[1]," into those with unrestricted dynamical possibilities and those with 'topographical' restrictions. Just as steam is prevented from leaving the cylinder by the walls, so are ions prevented from leaving solutions, or charges from the surfaces of conductors. All are physical systems. In each the distribution of process at any moment is determined by the number of reacting factors and forces.

It does not seem to us that the essential characteristics of the higher mental processes are in any way describable in the same terms. One of those characteristics is a true 'tendency' towards achieving some result and setting circumstances so that this result is achieved, however many external causal factors may try to prevent it. If I want to go from Cambridge to London, and start off by air, the aeroplane will get into a complicated set of cross-currents. Its tendency will be to move in the direction of the resultant vector of forces, which may point to Bath. But I control the aeroplane, and continually adjust its path. Then the engine fails, and I land in a field—but I shall get to London eventually. In *spite* of numerous deflecting causal factors, the object is achieved. In what way is this process comparable to the redistribution of charges that takes place when a certain amount of charge is taken from one plate of a condenser?

How do I see a circle according to Köhler? In the brain there wil
be two processes, corresponding more or less to the circle and to
surroundings. Between these two there is a dynamical mediator,
there may be two molecular concentrations in the brain-field, be
which a potential difference is set up. Why do I still see a circ
the illumination is changed, and the circle painted green?
although we now have two different concentrations, the
difference has remained the same. It is quite inconceivabl
how a circle could be given to me in direct experience if I o
local, indifferent, machine-conducted processes. But it is imm
obvious why a potential difference can and *must* give me the di
experience of a circle! We fail to see why the one should be so much more obvious than the other.

There is another ground on which the assumption, that our experi
ences are based on purely physico-chemical *Gestalten*, can be shown to
false. This has been fully worked out by Jaensch[2]. It is the falla
substituting causal for ontological analogies. The argument may b
be put thus: widely different spheres can be shown to have a l

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[1] P. 112.

[2] *Op. cit.*

number of corresponding properties. We have just seen that every physical system must necessarily conform to the two Ehrenfels criteria. But these are merely the expression of mathematical properties such as proportionality, which are shared by extremely diverse manifestations of the material universe. Certain fundamental mathematical functions recur, for instance, in the kinetic theory of gases, the theory of damped vibrations, the rate of growth of trees, the theory of economics, and the Weber-Fechner laws. Wherever the rate of increase of any system is proportional to the state of that system at each moment, some function of e (the base of natural logarithms) will occur, since the exponential function has the property that, if $y = e^x$, $\frac{dy}{dx} = e^x = y$, *i.e.* the increase dy of the function y is proportional to the original value. Would this fact, which to a superficial observer might appear to be of the most fundamental, if mysterious significance, lead us to suppose for one moment that there is any connection between the growth of trees and damped vibrations? No; the occurrence of such simple relations is a fact, not a necessity of thought, and it is the reason why mathematics can be the foundation for wide fields of knowledge, and why its conclusions are valid over a wide range. To argue that the different fields quoted above are connected, would be to argue on a false, *ontological* analogy.

A true, *causal* analogy is of a different kind altogether. To study the y of a liquid composed of oil and water past symmetrical sections foils) because that can be seen and photographed, and then to the knowledge thus gained to the flow of *air* past similar aerofoils d to aeroplanes, is to argue from a true, causal analogy, since factors that are operative are the same in kind. No physicist es von Ehrenfels' criteria in two systems will come to on that they must necessarily have anything in common e relationships, which apply indiscriminately to *every* group ts. But that is what *Gestalt* theory does. *For the ontological gies that are expressed in von Ehrenfels' criteria are taken to be a proof hysico-physiological and psychological* Gestalten *have a necessary inner hip, and that the structure of mental processes is that of these— ypothetical—physiological processes.*

mentioned before how much use Köhler makes of 'tendencies.' sty man experiences a feeling of pleasure when drinking a glass of on a hot day. The layman feels at once, Köhler says, that the sure is associated with the beer and the coolness of it, and not with uzzing flies, the orchestra, a smell of garlic, the bustling waiters or anything else; it is the direct outcome of cool beer and parched throat. Now let A = the process underlying the direct experience of coolness and taste, B = the process underlying experienced thirst. Both processes take place "in that part of the brain-field which corresponds to the self." The pleasure is the direct outcome of an immediate influence exerted by process A on process B. The processes corresponding to the experienced sounds from the orchestra or clattering plates happen not to have any tendencies to move in the direction of A or B[1]! Whatever happens in experience, it is the outcome of some tendency, just as, *e.g.*, condensers have the 'tendency' to re-establish a uniform distribution of charges after some charge has been withdrawn from one plate. That the two types of 'tendency' have nothing to do with one another is immediately obvious if we ask the following question: Would the re-establishment of a uniform distribution take place *in spite of* opposed causal factors? If it would, then if one of the plates of the condenser were bent, it should *of itself* bend straight again, since a uniform distribution is only possible over parallel plates. This does not happen, because the distribution has nothing whatever to do with any tendency; the form of the plates is the only condition, whereas the distribution is an invariant towards such external factors as the taking away of a definite amount of charge.

Throughout the book Köhler is at great pains to dissociate himself entirely from the older materialism in psychology. But it does not see to us that he has succeeded either in escaping from the older vi by enunciating radically new psychological principles, or in app other than purely physical principles, which have nothing to d psychology as such. Let us take the latter contention first.

The formal structure of all experience—and in this we mu association—is the same as that of the dynamic units on based. The organization of these units is *produced* by dynam it is due to the relative properties of the interacting physic cesses. But we have seen that this 'radical physiological hypot based on a false *ontological* analogy. The specific *Gestalt* prope enunciated by Köhler are common to the whole material—and m matical—universe[3]. But on this view organization must be fortuitous, and we are back at the biological mechanics of Weissm which Bernard Shaw caricatures so brilliantly in one of his prefa

[1] Cf. chapter on "Insight," in particular, pp. 375–376; 389–390.

[2] Even in the new non-commutative Algebra of Darwin and Dirac von Ehr properties hold.

[3] *Back to Methuselah.*

Meaning, according to Köhler, follows the lines laid down by natural organization. It is curious that this should generally be of biological significance, while the organization of physical systems is not. "Where organization is naturally strong we have spontaneous association; where there is practically no organization association does not occur until some organization is created intentionally[1]." The first part of this sentence admirably describes the fortuitousness of the process. *But the second part touches the vital psychological point.* In physical systems there is never any such *intentional* organization. It is characteristic of the mind and no amount of physico-physiological theorizing will locate this intention, a real psychological 'tendency,' in the dynamic 'structure' of physiological brain-processes.

Let us return to our first contention, that nothing radically new has been proposed. The principle of 'naïve description' (p. 76) always has been and must necessarily be a primary postulate for all psychological work except, perhaps, in work on conditioned reflexes or purely physiological and chemical events. Even in experiments on physiological after-images we must believe what the subject tells us.

The 'first primary psychological postulate' (p. 77) seems to be introspection in a slightly different form. For does not introspection use 'direct experience' "in order to guess at processes about which we wish formulate a working hypothesis"? We may admit that introspection often changes the field, and that introspectionists nevertheless e these changed phenomena to be the true ones. But the point or belief, remains the same.

psycho-physical theorem,' on the other hand, argues in a circle sis of a non-existing intermediary link. We do not construct world merely by uncaused endogenous acts of thought. certain aspects of reality, control the various operative n, and observe the results. This method gives constructive e cud for philosophical rumination, which finally produces : our picture of the physical world. The intermediary here is direct experience (of scales, etc.). But we cannot construct the "physiological processes directly underlying experience" in the same ay, since there is no further intermediary. We could only construct as part of *physical* reality by isolating them as such; and the work rian on the eel's retina, of Lashley on cerebral localization in rats, f many other physiologists has shown that the physiological pro- corresponding to some definite 'direct experience' cannot be isolated. Any one stimulus can give rise to completely different ranges of experience, because of 'the autonomy of organic processes.'

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[1] P. 290.

About the other axioms and inferences it is needless to say more. They are hypotheses and as such have been well presented by Köhler. In his preface he apologizes for his inadequate presentation owing to linguistic difficulties. We can only congratulate him on his success, and wish that many English and American psychologists could write as clearly and incisively. The difficulty of the book is not the language, but the hypotheses. It is good to insist, as Köhler does, that until really adequate quantitative methods are found, psychology should be content with qualitative observation and should not be bound by inflexible theories taken from physiology or physics. But in this book there is no detail whatsoever of apparatus or controls; qualitative observations are few and chiefly prove that the perception of wholes is a primary fact—which is widely realized in present-day psychology.

OBITUARY.

PROFESSOR MARY WHITON CALKINS.

THE news of the death of Prof. Mary Whiton Calkins on February 26th was received in this country with great regret.

For more than forty years Prof. Calkins had been associated with Wellesley College, Mass. From 1898 to 1929 she held the chair of philosophy and psychology, and in 1929 she was elected to a research professorship. She was a pupil of Prof. William James and of Prof. Münsterberg, and was greatly influenced by the philosophic teaching of Prof. Royce. She has stood as the exponent of personalistic or self-psychology and the value of her work has won widespread recognition. She received the honorary degree of Litt.D. from Columbia University in 1909 and that of LL.D. from Smith College in 1910. Her last visit to England was in December 1927, when on the invitation of the University of London she gave a short course of lectures. Consequent on this visit she was elected an Honorary Member of the British Psychological Society, and is the only woman to have received this honour.

Prof. Calkins made many important contributions to philosophical
nd psychological journals. An article by her, "The Limits of Genetic
Comparative Psychology," appeared in the first volume of this
nal. She contributed an article, "McDougall's Treatment of Experi-
" to vol. XIII, and a very characteristic article, "Converging Lines
emporary Psychology," to vol. XVI. Her two psychological
s. *An Introduction of Psychology* and *A First Book in Psy-*
her book *Persistent Problems in Philosophy* have been widely
ents in this country. As a teacher Prof. Calkins not only had
clear exposition but also the power of arousing in others the
asm she herself felt for her subject. She was an intelligent
c, scrupulously fair, and keenly alive to difference in standpoint.
She took an interest in the social and political problems of the present
y and applied to practical life the principles underlying her philo-
hy. In this she was a worthy representative of a family directly
nded from the *Mayflower* pilgrims. Miss Calkins' personal qualities
he least of which was a sense of humour—endeared her to all who
er. She will be greatly missed by a wide circle of friends.

B. E.

PUBLICATIONS RECENTLY RECEIVED

Principles of Experimental Psychology. By Henri Piéron. London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1929. Pp. viii + 186. 18*s.* 6*d.*

This is an exceedingly valuable book which ought to be readily accessible to every student of psychology. Although it contains a great amount of well-selected and well-attested information, it is not a text-book in the ordinary sense of the word. It will however powerfully help the student in his most difficult task, that of attaining an orientation towards the mass of often conflicting data and opinions that go to make up contemporary psychology. Prof. Piéron has set about his task in no narrow spirit, and he includes in his study historical notes and no small amount of information about human reactions in general, emotional and affective as well as cognitive, social as well as individual and clinical as well as experimental in the narrower sense. On every one of his topics he writes lucidly and concisely, and he is throughout interesting to a high degree. In short the volume deserves a really warm welcome on the part of all those who take psychology seriously. The translation by Dr J. B. Miner is excellent.

The Common Sense of Dreams. By H. G. Watt. Worcester, Massachusetts: Clark University Press (London: Humphrey Milford). 1929. Pp. xvi + 212. 13*s.* 6*d.* net.

In one respect at least this book serves to emphasize the great loss to English psychology which was suffered by the early death of its author. *The Common Sense of Dreams* is incomparably more direct and clear than any other published work o Dr Watt. At the same time its originality is less marked and is on the whole of rather derivative kind. The book begins by a detailed study of some of Rive published dreams and proceeds in an atmosphere of concrete analysis. Watt, most of his predecessors in this field, seemed to want a formula for the psycholo explanation of all dreams. He was however dissatisfied with the notion o fulfilment and also with that of 'conflict' mainly, apparently, because of played in them by hidden and inscrutable factors. His own formula is that is "the solution of some reluctance or reluctances." That is to say a train is started in waking life which is for some reason held up. The drea setting (also coming from waking experience) through which the 'hel can work. With this general principle in mind Watt gives examples pretation, discusses reflection in the dream, attacks nightmares, a good deal of forceful criticism of other theories. The book is well na have common sense—guided naturally by a lively and accurate knowledg chology—and nothing but common sense throughout. It is well worth reading in particular many of his criticisms are extremely well directed, while they are alw both serious and good humoured. Dr Shepherd Dawson contributes a brief sketc of Watt's career, and a bibliography of his publications.

Experiments in General Psychology. By Norma V. Scheidemann. Chic University of Chicago Press. 1929. Pp. 108. 4*s.* 6*d.* net.

This simple 'note-book manual' for the student describes a considerable of experiments of a psychological character all of which can be carried out elaborate apparatus. It should be of considerable interest to anybody who w know something in a general way about psychological experiments, and introduction to a more thorough treatment may serve a useful end.

Developing Appreciation through Teaching Literature. By Angela Marie Broening. Johns Hopkins Studies in Education, No. 13. Baltimore: Johns Hopkins Press. Pp. ix + 118. $2.00.

Dr Broening's problem was "How to teach literature so that boys and girls will love it and voluntarily read it for the satisfaction of the literary experiences it affords." With great energy and obvious enjoyment she attacked this problem in an experimental manner, and her monograph gives a lively and well-illustrated account of the results. She claims to have found a way: the enthusiasm of the teacher plays a great part. The monograph is full of interest and should be of practical value as well.

Academic Progress: A Follow-up of the Freshmen entering the University in 1923. By Harold A. Edgerton and Hubert A. Toops. Ohio State University Press. 1929. Pp. x + 150.

This is a detailed statistical study of the progress through the University of Ohio of a group of students throughout a period of four years. It appears that persistence is not accurately predicted by any of the measures commonly used for predicting academic success. The best 10 per cent. of students (as selected by Intelligence Tests) give good results, and the worst 10 per cent. bad results relatively speaking, but apparently in the case under study the difference of academic attainment falls considerably below that of intellectual promise.

The Anatomy and Physiology of Capillaries. By August Krogh. Yale University Press. 1929. 18*s*.

This book contains lectures given in America in 1922 and now brought up to date (July 1928, although published in 1929). Obviously the work is intended for histologists and physiologists, but no psychologist interested in skin sensibility can afford o remain in ignorance of it. Krogh shows how many are the reactions in the skin to muli which we interpret as different kinds of sensation. It is quite possible that ctate sensibility will turn out to be a matter of loci favourable for the eliciting of kind of skin reaction rather than another and not to depend on the presence of fic nerve endings. This field of inquiry is new and agreement on all points of tance for the psychologist has not yet been reached. Krogh's work should, e, not be accepted without reading as well the book by Sir Thomas Lewis, *d Vessels of the Human Skin and Their Responses*, London, 1927.

k of Psychiatry for Students and Practitioners. By D. K. Henderson D. Gillespie. Second Edition. Oxford University Press. 1930. 526. 18*s*. net.

ppearance of a second edition of this work is very welcome. It is in est general text-book in psychiatry that has been published in this ether for practical or theoretical purposes. The authors are thoroughly he psychological side of their topic and are as clear, precise and informative would be possible to be within reasonable limits of exposition. Nobody who has to do with the training of medical students on the functional side can fail to have ed the growing interest in and capacity to tackle the mental problems of medical which have been manifested during the last few years. For the further developf this interest and capacity no better book than this—so far as its specific go—could be desired. The second edition has not been changed very much, s been brought up to date by a few necessary additions.

roses. By I. S. Wechsler. London: H. B. Saunders Co. 1929. 330. 18*s*.

is a clearly written and most useful general manual. It contains a brief historical tion followed by chapters dealing with the 'mental mechanisms' that are of importance for the psychopathologist, with 'the etiology of the neuroses';

with 'classification of the neuroses'; with 'clinical manifestations of the neuroses'; with the 'diagnosis, course, and prognosis of the neuroses;' and with 'the treatment of the neuroses.' An appendix deals mainly with more immediately practical matters, concerning the history and examination of patients, general intelligence in relation to psychological disorders and the use of psychometric tests. A short Bibliography is added. The book cannot claim any very marked originality but it is written from a sane psychological point of view, the case material is sufficient and well chosen, and the exposition is straightforward. Taken all in all, it is a reliable and sensible book.

La Rève. Par MARTIN GOMES. Rio de Janeiro: Rodrigues and Co. 1928. Pp. 179.

This study of dreams considers their main stimulating causes to be various psychological tendencies which are separately discussed. A great many examples of dreams are given and considered, and the author shows a wide knowledge of contemporary literature. While nothing very new emerges from his study, he has collected a great amount of interesting data and interprets them in a sane manner according to approved psychological principles. The book is badly printed and contains numerous typographical errors.

Problems of Neurosis. By ALFRED ADLER. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1929. Pp. xxxvii + 178. 8*s*. 6*d*. net.

In this book Adler expounds his views simply and by the help of a number o case histories. He adds little or nothing to what he has said before, but writes with great clarity in a manner that will, beyond doubt, appeal to the general reader. In fact this is the best book that has appeared so far to recommend to the generally educated person who wishes to learn Adler's views. There is an appreciative and able preface by Dr F. G. Crookshank.

Psycho Analysis and Art. By GEORGE WHITEHEAD. London: John Ba Sons and Danielsson, Ltd. 1930. Pp. 146. 5*s*. net.

Part I is concerned with anarchy and authority. It detects and deplores a gro anarchy in psychology, in sex relations, and in industry and politics. It insist repression is called for in all these fields. Part II deals with sex and decorati with the wonders of instinct and with the wonders of intelligence. Noth emerges from the discussion. Part III deals scrappily and with much quot a diversity of topics: day dreams, racial phantasies, the evolution and art. In general artistic gratification is for the purpose of sublimating " and to generate fervour for personal and social sacrifice." Part IV Psychology of the Penny Dreadful," but it deals with a large numb does little more than repeat that what is chiefly needed in presen the satisfactory sublimation of crude instincts. The whole book lac organization and is disappointing.

The Art of Interrogation. By E. R. HAMILTON. (Introduction by C. SPEAR London: Kegan Paul, Trench, Trubner and Co., Ltd. Internation Library of Psychology, etc. 1929. Pp. xii + 171. 7*s*. 6*d*. net.

Most examiners and nearly all examinees at some time or another doubt the v of examinations. In this decidedly interesting book they may find many reasons for doubt clearly stated and also some reasons for belief as admira forward. The whole discussion is in fact interesting and attractive, and proceeds upon a rather general plane for the most part, is obviously the o much thoughtful concrete exploration of its topic. The sub-title of the book is in the principles of mental tests and examinations." This accurately indi subject-matter, but hardly indicates how positive is the value of Mr Ha contribution. Every cautious psychologist must now admit that the unt multiplication of 'tests' for every conceivable reaction is one of the greatest dangers of his rapidly developing science. Mr Hamilton has thought well and worked well about this, and as a result he not only puts forward admirably pointed criticism but he also indicates how the 'test movement' may develop along sound lines. Prof. Spearman's appreciative introductory note is thoroughly justified by the book's performance.

Philosophie der Symbolischen Formen. Von Ernst Cassirer. Dritter Teil: Phänomenologie der Erkenntnis. Berlin: Bruno Cassirer. 1929. S. 572. M 22.

It is impossible here to do more than mention the appearance of this enormous volume, which should be very carefully studied by all those who are primarily interested in the epistemological implications of psychology. The book contains three main sections, dealing with: ***Ausdrucksfunction und Ausdruckswelt; Das Problem der Repräsentation und der Aufbau der anschaulichen Welt,*** and ***Die Bedeutungsfunction und der Aufbau der wissenschaftlichen Erkenntnis.*** The whole is an extremely able and learned treatise, though many readers will find it far from easy to make the necessary transitions from experimental and empirical data to far reaching epistemological formulations.

Foundations of Geometry and Induction. By Jean Nicod. London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1930. Pp. 286. 16*s.* net.

Psychologists with a taste for epistemology and logic will enjoy this very able volume. Nicod attempts to begin with data of perception and from these to build up various geometries which can be said to be based upon them. Naturally the results often seem to have little relation to the concrete and complex facts of everyday life;
ut the argument is always stimulating and vigorous.

Nature of Life. By Eugenio Rignano. London: Kegan Paul, Trench,
Trubner and Co., Ltd. International Library of Psychology, etc. 1930.
p. x + 168. 7*s.* 6*d.* net.

s the most complete and concise exposition by Rignano of his views that has
n English. In it he applies his contention that the explanation of all living
ust include a demonstration of determination by purposiveness to a wide
nomena, from those of elementary physiological functions to those of
l orders. He also maintains that his view represents a half way house
m and mechanism, having the excellencies of both, the drawbacks of
ther values all its own.

sche Anlage der Jugendlichen in ihrer Beziehung zur Künstlerischen
estaltung. Von Dr Paul Metz. Langensalza: Herrmann Beyer und
Söhne. 1929. S. 114. 2,70 R.-M.

The eidetic images of children are found to be on the whole extraordinarily plastic.
particular, the form is more easily changed than the colours in the case of 'basedow'
es; the eidetic images of 'tetanoid' children are more rigid. But in both cases
attempt to copy them by running a pencil along them failed, because the
move in the direction in which the pencil moves, and are distorted in various
e author shows that the act of drawing of the child is based on dynamic, or
tterns, and is a kind of language. The ideoplastic schema of children's
s is developed at the same time as language, which is proved by the fact that
wings of deaf and dumb children are physioplastic. Nevertheless, eidetic
have a most important function in providing the child with picture material,
can be moulded in various ways.

Die Psychologie des Grundschulkindes in ihrer Beziehung zur Kindlichen Gesamtentwicklung. Von Oswald Kroh. Langensalza: Herrmann Beyer und Söhne. 1930. S. 345, 6,60 R.-M. 4th–6th edition.

This book provides a clear exposition of the various phases in the development of the child, and analyses the development of ego-experience, perceptions, thinking, imagination, and the growth of ethical, religious, aesthetic and social experiences during the primary school age. An attempt is made to classify school children into the various types of Kretschmer. The author pleads for a far wider basis of education in the primary schools, because the plasticity and receptivity of the child at this age give it unlimited potentialities, if these are stimulated at the right time.

Über Gestaltpsychologie und Gestalttheorie. Von Prof. Dr E. R. Jaensch und Dr L. Grünhut. Langensalza: Herrmann Beyer und Söhne. 1929. S. 167. 5,40 R.-M.

The authors have given a lucid, critical analysis of the basis on which *Gestalt*-psychology rests. They show that it is a thoroughgoing materialism, veiled behind a misleading terminology and fundamentally false assumptions about the nature of the physical world. The physical *Gestalten* on which all experience and ideas are supposed to rest are shown to be merely physical systems, whose 'behaviour' i completely predictable from the nature of their parts; they are not teleologica 'wholes' at all. Moreover, *Gestalt* psychology has misunderstood the meaning of th Kantian categories, and substituted ontological for causal analogies.

Empirisch-psychologische Beiträge zur Typologie des dichterischen Schaffens Von Dr Berthold Leinweber. Langensalza: Herrmann Beyer und Söhne. 1929. S. 96. 2,40 R.-M.

A psychological analysis was made of the working methods of a group of writers by means of experimental tests and an examination of the type of symbolism eac employs. On the results of these tests they are divided into three types: the dynami the naive or 'coherent,' and the contemplative. The results are also compared w the more general classification of the Marburg School into various 'integrate' ty

Die Lehrbarkeit der Religion. Von Dr Emil Schorsch. Langensalza: He Beyer und Söhne. 1929. S. 96. 2,10 R.-M.

The author defines the nature of religion as the reciprocal activity b individual and objective Being. The child can be taught to experience t the teacher's religious life is in all respects an inner unity, and provid taken of three types of religious expression: intellectual, voluntaristic

Wirklichkeit und Wert in der Philosophie und Kultur der Neuzeit. Jaensch. Berlin: Otto Elsner. 1929. S. xvi + 251.

This work is intended as *prolegomena* to a reconstruction of philosoph basis of philosophical anthropology and *Wirklichkeitsphilosophie.* The adv naturalistic positivism, and its attempt to explain everything, including conscio ness, on purely mechanical principles, forced philosophy to retire more and more in 'unreal' spheres, to seek the peculiar functions of consciousness in some transc dental basic world underlying physical phenomena. But both positivism and idea were bound to fail, since the one banished values and tendencies towards ach and realizing such values from the universe because they are not to be f inorganic nature, while the other sought them in unreal spheres. This speculative endeavour is critically examined, and its limitations are set fo are found to have their origin in Cartesianism, and this is shown to be limite experience and the mode of thinking of a certain psychological type. Since o of experience and of thinking are bound by our type, a reconstruction of the t categories becomes necessary.

Grundformen menschlichen Seins, mit Berücksichtigung ihrer Beziehungen zur Biologie und Medizin, zur Kulturphilosophie und Pädagogik. Von Prof. Dr ERICH JAENSCH (und Mitarbeitern). Berlin: Otto Elsner. 1930. S. 524, 42 illustr. 23 R.-M.

The long series of researches on eidetic imagery carried out in the Marburg Institute led Jaensch and his pupils into the psychology of types. It was found that certain fundamental modes of response were made by the same subject in every experiment and that the same general psychological structure was traceable from the lowest levels (physiological after-images, psycho-reflex of the pupil) to the highest (synaesthesias, the will, formation of concepts and values). In this volume a number of researches are collected, whose aim was the analysis of 'integrate' and 'disintegrate' forms of personality.

In the first part Jaensch shows the necessity for a thorough psychology of types and its application to philosophy, medicine and paedagogy. He discusses the experimental facts that have led him to formulate the concept of 'integration' and 'disintegration' as regards the various structural levels of consciousness, and shows that, from a biological point of view, the integrate type is a fundamental one. What is interesting about his methods is that it now becomes possible to combine rigorously experimental work with biographical and 'understanding' or 'interpretive' psychology, to give a complete picture of the whole structure of a personality. The study of such a personality as a type can then throw much light on questions of race psychology as is shown in the appendix, where Jaensch gives a psychological character-sketch of Stresemann.

The other parts of the volume are experimental researches by various collaborators, on pronounced cases of the *B*-type and integrate types generally; on the intermediary and transition types from integration towards disintegration; on synaesthesias and the type of personality correlated to them; and on the psycho-reflex of the pupil, a particularly accurate piece of work.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

March 8, 1930.	"Children's Thought." By Miss V. HAZLITT, D.Litt.
,, ,,	"Imagery and Learning." By Miss A. M. JENKIN, B.A.
April 26, 1930.	"A Critical Examination of Kohler's Gestalt Psychology." By Dr O. A. OESER.
,, ,,	"Psychological Factors in Peripheral Vision." By Mr G. C. GRINDLEY.
,, ,,	"A Photographic Study of Eye Movements in Reading." By Miss M. D. VERNON.

SECTIONS.

MEDICAL.

January 29, 1930.	"Diagnostic Significance of Sensory Aurae Epilepsy." By Dr JOHN MACCURDY.
February 26, 1930.	"On the Psychology of Values." By Dr M. D. EDER.
March 11, 1930.	(*Joint Meeting with Psychiatry Section.*) A Symposium o "The Psychotherapy of the Psychoses." By Dr DEVINE and Dr EDWARD GLOVER.
April 30, 1930.	"Some Points of Disagreement with Freudian Practice and Theory." By Dr HILDA M. WEBER.

EDUCATION.

January 7, 1930.	(*Joint Meeting with Nursery School Assoc.*) "Principle Practice in Nursery School Education." By Mrs ISAACS, M.A., and Miss LILLIAN DE LISSA.
February 3, 1930.	"An Investigation into the Relation between Intelli Inheritance." By Miss EVELYN LAWRENCE, Ph.D

AESTHETICS.

March 13, 1930.	Prof. LASCELLES ABERCROMBIE upon his Essay on Art."

INDUSTRIAL.

February 25, 1930.	"Some American Approaches to the Study of Temperame By MARION MILNER. B.Sc.

JOINT MEETINGS.

EDUCATION AND INDUSTRIAL.

March 24, 1930. Papers on: "The Relation of Raising the School leaving Age to Problems of Vocational Guidance and Selection," contributed by Miss SHEILA BEVINGTON, B.Sc., from the point of view of Vocational Guidance,—and "Thomas Tibbey," from the point of view of Education.

EDUCATION AND AESTHETICS.

May 5, 1930. "The Child's Innate Sense of Music." By WILLIAM PLATT.

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Adelaide, South Australia.
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Alton, Mrs Ellen S., B.Sc., 22, Holmbush Road, Putney Hill, S.
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M. Anderson, E. W., M.B., Ch.B., D.P.M., City of London Mental Hospit
Stone House, near Dartford, Kent.
M.E.I. Anderson, W. L. G., M.B., Ch.B., 93, Harley Street, W. 1.
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Andrews, Miss E., B.A., Director of Personnel, State College for
Tallahassee, Florida.
E. Andrews, V. W., St Valentine's, 14, Park Road, Cheam, Surrey.
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Bartlett, Mrs E. M., 2, Millington Road, Cambridge.
E.I.Ae. Bartlett, F. C., M.A., Psychological Laboratory, Cambridge.
M.E.I.Ae. Bartlett, R. J., M.Sc., A.R.C.S., 3, Eastcote Road, Harrow.
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I. Beaver, C., 1, Bell Place, Stamford Road, Bowdon.
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Beeby, C. E., M.A., Ph.D., Canterbury University College, New Zealand.
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I. Bennet, E. A., M.A., M.B., D.P.M. (Physician, Tavistock Clinic, Clinical Assistant Bethlem, late Clinical Ass. Maudsley), 42, Harley Street, W. 1.
Berg, G. C., M.D., 5, Harley Street, W. 1.
Berkeley Hill, Lt.-Col. O. A. R., I.M.S., M.A., M.D., Medical Superintendent, Ranchi European Mental Hospital, Ranchi (Bihar and Orissa), India.
Berlyn, J. A., M.B., 19, Greenfield Crescent, Calthorpe Road, Edgbaston.
Bernstein, E., 49, Cable Street, E. 1.
Bevington, Miss S. M., B.Sc., National Institute of Industrial Psychology, Aldwych House, W.C.
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ckersteth, Miss, 32, Stafford Street, Edinburgh.
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.
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	Burke, J. B., M.A., 63, St James's Street. S.W. 1.
M.	Burke, N. H. M., M.R.C.S., L.R.C.P., D.P.M. (Lond.), Fountain Ment Hospital, Tooting Grove, S.W. 17.
M.E.I.	Burt, Prof. C., M.A., D.Sc., 30, Princess Road, Regent's Park, N.W. 1.
M.E.	Buysman, Miss H. C., Elsinore, 12, Daysbrook Road, S.W. 2.
	Caldicott, Charles H., M.B., Grantbourne, Chobham, Woking.
E.	Cameron, Miss Annie E., M.A., 24, Langbourne Avenue, Highgate, N
E.	Campbell, Miss I. E., M.A., B.Ed., University College, Reading.
M.	Carr, Dr Ruby Thornton, Horton Mental Hospital, Epsom, Surrey
	Carr, W. B. H., Junior United Service Club, Charles Street, S.W.
M.	Carroll, Dr Dennis, 6, Tavistock Mansions, W. 1.
M.	Carver, A. E. A., M.A., M.D., Caldecote Hall, Nuneaton.
	Cattell, R. B., Casaletta, Marine Drive, Paignton.
	Caughey, J. W., 73, St John's Park, Blackheath, S.E. 3.
	Cavenagh, Prof. F. A., M.A., Singleton Park, Swansea.
M.E.	Chadwick, Miss M., 48, Tavistock Square, W.C. 1.
I.	Chambers, E. G., M.A., 38, Old Queen Street, S.W. 1.
E.	Chapman, Albert E., M.A., Department of Education, Birmingham.
E.	Charles, Albert, M.A. (Lond.), 91, Penchwintan Road, shire.
M.	Chatterton, Dr Harold, M.R.C.S., L.R.C.P., 22, Weym
M.	Chevassut, Rev. F. G., M.A., B.Sc., Hulme Church Hostel, Manchester.
E.	Claremont, C. A., B.Sc., Studio House, Rosslyn Hill, Hampste
	Clarke, E. R., B.A., 57, Bertrand Road, Bolton, Lancs.
E.	Clarke, Miss W. S., Ashburne Hall, Fallowfield, Manchester.
E.	Clarkson, Miss M., St Mary's College, Cheltenham.
M.	Clegg, E. A., M.R.C.S., L.R.C.P., Greeba, Curzon Avenue, Beacons Bucks.
M.	Cloake, P. C. P., M.D., B.S., B.Sc., M.R.C.P., D.P.M., Mart Pakenham Road, Edgbaston, Birmingham.
E.	Coetzee, J. Chr., M.A., M.Ed., D.Phil., Professor of Education, Reit Potchefstroom, S. Africa.
	Coit, S., Ph.D., 30, Hyde Park Gate, S.W. 7.
M.	Coleman, Stanley M., M.R.C.S., L.R.C.P., D.P.M., Dorset Mental Herrison, Dorchester.
E.	Coles, P. B., M.A., 148, West Hill, Putney, S.W. 15.
M.E.	Collins, Miss M., M.A , Board of Education, S.W. 1.

M.E.I. Collins, Miss Mary, M.A., B.Sc., Psychological Dept., The University, Edinburgh.
I. Collis, Prof. E. L., M.B.
E. Comline, Robert, B.A., Executive Officer, Roxburgh Education Authority, Roxburgh.
Conn, Rev. J. C. M., M.A., Regent Crescent, 220, Battlefield Road, Langside Glasgow.
M. Connell, E. H., M.B., Ch.B., 7, Greenhill Gardens, Edinburgh.
M. Cook, L. C., B.A., M.R.C.S., L.R.C.P.
Cooke, Miss K. S., B.A., 1, Sherbrook Gardens, N. 21.
M.E. Cooper, J. W. A., L.R.C.P. & S.
M. Core, D. E., M.D., 26, St John Street, Manchester.
Cousin, D. R., B.A., The University, Glasgow, W. 2.
E. Cox, J. W., D.Sc., 184, Northumberland Park, Tottenham, N. 17.
M. Crawford Scott, Helen, M.R.C.S., L.R.C.P., 122, Stamford Street, S.E.
I. Crombie, Nickel, B.Com. (Edin.), National Institute of Industrial Psychology, Aldwych House, W.C. 2.
M.E.Ae. Crosthwaite, Mrs A. A., 54, Hunter Street, Brunswick Square, W.C. 1.
E. Cull, E., B.Sc., Education Dept., University, Edmund St., Birmingham.
M. Culpin, Millais, M.D., F.R.C.S., 1, Queen Anne Street, W. 1.
Curtis, S. J., B.A., 58, Victoria Drive, Horsforth, Leeds.
Cynaeus, Mrs A., W. Chausseen 38e. Helsingfors, Finland.
I. Dainow, M., B.Sc., 21, Gower Street, W.C. 1.
Dale, Miss A. B., M.A., Ph.D., Old Hall, Newnham College, Cambridge.
Dall, A. W., M.A., Maisonette, Maybury Road, Woking.
. Darwin, Miss Ruth, The Orchard, Huntingdon Road, Cambridge.
Davey, Miss C. M., 67, Monmouth Road, Westbourne Park, Adelaide, S. Australia.
Davies, The Hon. Rev. Albert E., M.A., 14, South Parade, Whitley Bay, nr Newcastle-on-Tyne.
vies, Miss A. R. Lewis, B.A., 1, Royal Terrace, St David's, Pembrokeshire, S. Wales.
es, Rev. H. V., B.A., St Peter's Rectory, Bridgnorth, Salop.
on, F., M.A., 17, Upper Grosvenor Road, Handsworth Wood, Bir-ngham.
S., M.A., D.Sc., Hazel Bank, Bearsden, Dumbartonshire.
W. M., 9, Endsleigh Gardens, W.C. 1.
rof. W. S., M.A., M.D., M.R.C.P., The University, Sydney, N.S.W. ia.
M.A., 10, New Quebec Street, W. 1.
E. Margaret, 18, Ampthill Street, Norwich.
Miss E. R., B.A., Homestead, Marlborough Road, Romford.
H., M.D., F.R.C.P., The Ridge, Virginia Water, Surrey.
son, T. E., M.A., D.A., George Watson's Ladies' College, George Square, Edinburgh.
Dillon, F., M.D., Ch.B., Northumberland House, Green Lanes, Finsbury Park, N. 4.
Dixon, Major E. T., Billy Dun, Half Way Tree, Jamaica.
Dobson, R. R., The Grammar School, Cheltenham.
Donovan, Mrs E. M., M.A., University Women's Club, 2, Audley Square, South Audley Street, W. 1.
Dorward, A., M.A., The University of Liverpool, Liverpool.
Dove, Miss Janie Watson, M.A., Ed.B., Mayfield Club, 60, Harborne Road, Edgbaston, Birmingham.
Doyle, Sister M., M.A., 17, Victoria Circus, Hillhead, Glasgow, W. 2.
Drever, James, M.A., B.Sc., D.Phil., Psychology Department, University, Edinburgh.
Drummond, Miss M., M.A., 10, Hartington Gardens, Edinburgh.
Duff, Miss I. F. Grant, 4, Manson Place, S.W. 7.

M. Eager, R., M.D., The Devon Mental Hospital, Exminster.
E. Earle, F. M., M.Ed., B.Sc., The High School, Kirkcaldy, Fife.
M. Eddison, W., Wonford House, Exeter.
M. Eder, M. D., M.D., 16, Nottingham Place, W. 1.
E.I.Ae. Edgell, Prof. B., M.A., D.Litt., 15, Lyon Road, Harrow.
Edwards, J. O., B.Sc., Ph.D., 11–12, Wind Street, Swansea, S. Wales.
El-Nahas, M.A., L.Sc., Dip. of Ed. (Cairo), The University, Edmund Street, Birmingham.
English, Horace B., Antioch College, Yellow Springs, Ohio, U.S.A.
E. Ensor, Mrs Beatrice, 11, Tavistock Square, W.C. 1.
E. Evans, John, B.A.
M. Fairbairn, R. D., M.B., Ch.B., 18, Lansdowne Crescent, Edinburgh.
I. Farmer, Eric, M.A., Holy Trinity Vicarage, Carlisle Street, Lambeth, S.E. 1.
E.Aes. Farrar, Miss D. H., B.A., Heatherstone, Queen's Gate, Halifax, Yorks.
Farrow, E. P., D.Sc., Limehurst, Spalding.
Fazan, E. A. C., M.R.C.S., Belmont, Wadhurst, Sussex.
E. Feasey, Miss L., M.A., Trinity Hall, Duke Street, Southport.
E.I. Ferguson, R. W., B.Sc., A.R.C.S., 36, Linden Road, Bournville, Birmingham.
M.E. Fildes, Miss A. F., Hockerill College, Bishop's Stortford.
M.E. Fildes, Miss L. G., B.A., 9, Culverden Avenue, Tunbridge Wells.
E. Fitch, Arthur A., M.R.C.S., L.R.C.P.
Fleming, Miss C. M., M.A., Ed.B., 55, Campbell Street, Greenock.
M. Fleming, G. W. T. H., M.R.C.S., L.R.C.P., Dorset Mental Hospital, Herris Dorchester.
M. ● Flower, Rev. J. Cyril, M.A., 84, Chesterton Road, Cambridge.
M.E.I.Ae. Flugel, J. C., B.A., D.Sc., 11, Albert Road, N.W. 1.
M.E.I. Flügel, Mrs J. C., 11, Albert Road, N.W. 1.
M.E.I.Ae. Flügel, Mrs Mary, 54, Harley House, N.W. 1.
M. Flynn, Rev. Thos. E., M.A., St Joseph's College, Upholland, Wiga
Forster, L., M.A., B.Sc., The University, Hong Kong.
M.E. Forsyth, D., M.D., 21, Wimpole Street, W. 1.
E.Ae. Foster, Miss I. M., B.A., Homestead, Marlborough Road, Romf
M.E. Fowler, H. L., M.A., Ph.D., The University, Perth, Western
E. Fox, C., M.A., Warkworth House, Cambridge.
M.E. Fox, Miss Evelyn, Central Association for Mental Welfare, 2 Palace Road, S.W. 1.
E Fox, Miss M. E., County School for Girls, Beckenham, K
M.E. Fox, Miss M. G., c/o Barclays Bank, Ltd., Queen's Road,
Fox, S. J., Box 3541, Johannesburg, S. Africa.
M. Franklin, Dr M. E., M.B., B.S., 86, Harley Street, W.
E. Franks, Miss H., 34, Brunswick Square, W.C. 1.
Fraser, J. A., M.A., B.Ed., 29, Glendale Avenue, Burnage,
M.E. Frodsham, Miss M., B.Sc., 22, Somerset Terrace, W.C. 1.
Fullegar, Miss J., M.A., Episcopal Training College, Dalry H Place, Edinburgh.
M.E. Gabler, Mrs Rosalie. 129, King Henry's Road, N.W. 3.
Gallie, Miss J. B., M.A., Training College, Jordanhill, Glasgow.
Ganeva, R., Ph.D., Bd. Dondourov, 97, Sofia, Bulgaria.
M.E. Gans, Mrs Howard, 25, East 77th Street, New York City, U.S.A.
M. Gardner, Mrs E. A., B.A., Stamford House, Wimbledon Common, S.W
Garnett, A. C., M.A., Litt.D.
E. Garnett, J. C. M., M.A., 21, Well Walk, N.W. 3.
Gaw, Miss Frances, B.A., Ph.D. (Lond.), Child Study Dept., Seatt Schools, Seattle, Washington.
E.I. Gems, J. F., Eaton Mount, Cobham, Surrey.
Gibson, A. B., M.A., 165, Balden Road, Harborne, Birmingham.
M. Gibson, H. C., M.B., 24, Dick Place, Edinburgh.
M. Gillespie, C. M., M.A., The University, Leeds.
M. Gillespie, R. D., M.D., Ch.B., D.P.M., 25, Upper Wimpole Street, W

Ginsberg, M., M.A., London School of Economics, Houghton Street, Aldwych, W.C. 2.

M.E. Glaister, N., M.B., B.S. (Lond.), Northumberland House, Finsbury Park, N. 4.

M. Gleaves, E., M.Sc., M.Ed., M.B., Ch.B., St John's Chambers, 2, St John's Street, Manchester.

M.E. Glover, Edward, M.D., 18, Wimpole Street, W. 1.

M.E. Good, T. S., M.R.C.S., L.R.C.P., Ashhurst Special Neurological Hospital, Littlemore, Oxon.

I. Goodall, George, B.Sc. (Econ.), Lond., 8, Aberdeen Court, Highbury, N. 5.

E. Gordon, Hugh.

I. Gordon, Margaret H., M.D., c/o Messrs McVitie and Price, Harlesden, N.W. 10.

M. Gordon, R. G., M.D., M.R.C.P., 9, The Circus, Bath.

Gosse, Miss E. T., 17, Hanover Terrace, N.W. 1.

Goulston, Mrs B., B.A.

E. Gowan, Miss Edith M., B.A., Principal, St Katharine's College, Tottenham, N. 17.

Grant, Miss C. R., 14, Greenhill Terrace, Edinburgh.

Green, G. H., Dept. of Education, University College of Wales, Aberystwyth.

M.E.Ae. Green, Mrs J. A., St Mary's College, 50, Lancaster Gate, W. 2.

Greenhill, E., 68, Glencairn Drive, Glasgow, S. 1.

Ae. Greig, J. Y. T., M.A., D.Litt., Armstrong College, Newcastle-on-Tyne.

. Griffith, Percy, M.I.C.E., F.G.S., F.R.S.I., 39, Victoria Street, S.W. 1.

Griffiths, Miss R., B.A., 12, Doughty Street, W.C. 1.

Grindley, G. C., Psychological Laboratory, Cambridge.

Hadfield, J. A., M.B., 9, Harley Street, W. 1.

Haire, Norman, Ch.M., M.B., 127, Harley Street, W. 1.

Halsey, Mrs Gladys, D.Sc., St Clair, Effingham Road, Surbiton.

Hamilton, E. R., University College of North Wales, Bangor.

amlyn, Mrs P., 38 a, Clanricarde Gardens, W. 2.

mmerton, Miss E., B.Sc., 51, Warrington Road, Harrow.

mond, J., M.B., Ch.B., 59, Queen Anne Street, Cavendish Square, .1.

nd, Miss Marjorie, B.A., Education Department, Birmingham iversity.

n, F. A., M.B., Blue Spur, Chesham Bois, Bucks.

tle, D. N., M.R.C.S., L.R.C.P., c/o Westminster Bank, Birkenhead.

L., M.A., B.Ed., 8, Laird Street, Coatbridge, Lanarkshire.

D. W., 1, Pendennis Road, Streatham, S.W. 16.

aves, Rev. H. L., B.A., B.Mus., The Clergy House, 2, Spencer Hill, W. 19.

argreaves, Miss L. J., M.Sc., Southlands College, Belmont House, Wimbledon Park Side, S.W. 19

Harms, Dr Ernst, c/o Karl Wallach, Alsfeld, Hessen, Germany.

Harper, R. S., M.R.C.S. (Eng.), L.R.C.P. (Lond.), F.R.M.S., Med. Supt., Min. of Pens. Neur. Clinic, Brighton, 36, First Av., Hove, Sussex.

Harris, U. A., M.A., c/o Miss Macdougall, 13, Bruntsfield Avenue, Edinburgh.

Hart, Bernard, M.D., 94, Harley Street, W. 1.

Hawtrey, Miss F., Avery Hill Training College, Eltham, S.E. 9.

Hayes, A. E , F.R.S.A., 52, Priory Gardens, Highgate, N. 6.

Hayes, E. D., M.B., B.Ch., D.P.M., Berry Wood, Northampton.

Hazelton, Richard.

. Hazlitt, Dr V., M.A., Bedford College, Regent's Park, N.W. 1.

Head, Sir Henry, M.D., F.R.S., Hartley Court, Reading.

Hearn, Miss E. G., Grey Hall, Avery Hill College, Eltham, S.E. 9.

Hemingway Rees, Dr Mary I., M.R.C.S., L.R.C.P., 14, Wimpole Street, W.

E. Hepburn, W. A. F., M.A., B.Ed., Director of Education, Education Offices, Wellington Square, Ayr.
M.E. Herford, Ethilde B. M., M.B., B.Sc., 19, Redlands Road, Reading.
I. Hichens, W. L., 3, Central Buildings, Westminster, S.W. 1.
M.I. Hicks, Prof. G. D., M.A., Ph.D., 9, Cranmer Road, Cambridge.
E. Hilda, Sister Marie, Notre Dame, Dowanhill Training College, Glasgow.
M.E. Hill, J. C., M.Sc., 56, Bushwood Road, Kew Gardens, Surrey.
Hingley, Rev. R. H., B.A.,
M. Hoernlé, Prof. R. F. A., M.A., B.Sc., The University of the Witwatersrand, Johannesburg.
E. Hollander, Bernard, M.D., 57, Wimpole Street, W. 1.
Holt, Miss M.
M.E.I.Ae. Hope, Thomas, M.A., B.Ed., 17, Wilton Road, Edinburgh.
M.E. Hopkins, Dr Pryns, M.A , 36, Rue Chardon, Lagache, Paris, France.
Horsfall, Miss T. E. P., B.Litt. (Oxon.), 1, Belbroughton Road, Oxford.
Howard. C. C., B.A., B.Sc., 37, Walsingham Road, Enfield.
M. Howden. R. A., 51, Tavistock Square, W.C. 1.
M. Howe, Eric, M.R.C.S., L.R.C.P., 1, The Woodlands, London Road, Harrow-on-the-Hill.
Howie, Mrs M., 8, Middleby Street, Edinburgh.
Huggins, Miss L. M., Nurses' Home, Brompton Hospital, Fulham Road S.W. 3.
E. Hughes, A. G., Ph.D., B.Sc., M.Ed., 45, Harvard Road, Isleworth.
I. Hughes, D. E. R., Fron Dêg, Mold, N. Wales.
E. Hughes, Mrs E. H., B.A., 45, Harvard Road, Isleworth.
E. Hughes, Miss M., B.Sc.
E. Hume, Miss G., M.A., 7, Holborn House, Baldwin's Gardens, Grays Inn W.C.
M. Hutchison, Dr Alice M., 28, Wimpole Street, W. 1.
M. Hutton, Dr L. F., B.A., M.R.C.S., L.R.C.P., Resident Quarters, Chari Hospital, W.C. 2.
M. Inman, William S., M.B., 22, Clarendon Road, Southsea.
E. Isaacs, N., 54, Regent's Park Road, N.W. 1.
M.E. Isaacs, Mrs S., M.A., 54, Regent's Park Road, N.W. 1.
E. Jackson, Miss A. M., M.A., Torrington, 48, Pollards Hill E S.W. 16.
M. James, G. Wm B., M.D. (Lond.), The Lawn, Hillingdon, Mi
James, H. E. O., B.A., The University, Manchester.
M. Jeffreys, Harold, M.A., D.Sc., St John's College, Camb
Jeffreys, M. V. C., M.A. (Oxon.), Armstrong College, Ne
E. Jenkin, Miss Annie M., B.A., 78 Alleyn Road, Dulwich,
E. Johnston, Miss Katherine L., M.A., Maria Grey Training Road, N.W. 6.
E. Jones, B. P., B.A., 15, Denmark Avenue, S.W. 19.
M. Jones, E., M.D., M.R.C.P., 81, Harley Street, W. 1.
E. Jones, Miss E., The Frogmore Mentally Defective School, Wand S.W. 11.
E. Jones, Miss E. W., B.A., 2, Fordington Road, Highgate, N. 6.
Ae. Jones, Miss V. G. A., B.A., 3, Holly Lodge Gardens, West Hill, Highgate, N. 6
E. Joseph, Miss C. M., 81, Holland Park, W. 11.
Kay, J., B.Sc., 139, Victoria Road, Old Charlton, S.E. 7.
E. Kay, L. J., 42, Wilson Street, E. 14.
Kaye, M., M.A.
E. Keatinge, M. W., M.A., Tommy's Heath, Boar's Hill, Oxford.
M. Kelly, Simon, B.Sc., M.R.C.S., L.R.C.P., 69, Dickenson Road, Ru Manchester.
Kemp, Peter, M.A., B.D., 6, Minard Road, Partickhill, Glasgow.
E. Kennedy-Fraser, David, M.A., B.Sc., Jordanhill Training College, Ch lain Road, Glasgow.

M. Kenneth, J. H., M.A., Ph.D., The Homestead, Clynder, Dumbartonshire.
I. Kent, A. F. Stanley, M.A., D.Sc., Holm Ray, Iron Acton, Glos.
E. Kerr, R. B., M.A., B.Ed., 45, Melville Street, Portobello, Edinburgh.
Kilgour, John, M.A., B.Ed., 98, Nether Street, Kirkcaldy.
M.E.I. Kimmins, C. W., M.A., D.Sc., National Liberal Club, Whitehall Place, S.W. 1.
King, Miss E. M., B.A., The University, Sheffield.
King, Rev. J. Leycester, Heythrop College, Chipping Norton, Oxford.
E. Kirkman, F. B., B.A., 16, Denning Road, Hampstead, N.W. 3.
E. Kittermaster, Rev. D. B., M.A., Grove Hill House, Harrow.
I. Klein, Major Adrian B., 32, The Pryors, East Heath Road, Hampstead Heath.
M. Klein, Mrs Melanie, 93c, Linden Gardens, W. 2.
Knight, A. R., M.A., The University of Aberdeen, Aberdeen.
M. Korn, Morris, 101, Canfield Gardens, N.W. 6.
M. Kornerup, Miss H. E., B.A., c/o F. S. McVitie, 15, New Street, Bishopsgate, E.C. 2.
M.E. Krause, R. A., M.D., D.Sc., Norwood, Kinghorn.
M. Ladell, R. MacDonald, M.B., Ch.B., 395, Coventry Road, Small Heath, Birmingham.
Laird, Miss Isabel M., M.A., B.Ed., 22, Dudley Gardens, Edinburgh.
Laird, Prof. J., Powis Lodge, Old Aberdeen.
Lall, Mrs D. M., The Dept. of Fine Art, Armstrong College, Newcastle-on-Tyne.
Laugher, J. H., Ravenscar, Sedgley Road, W. Tipton, Staffs.
Lavin, C. R., M.D., Sc.D., Lagasca No. 64, Madrid, Spain.
Lawrence, Miss E., B.Sc. (Econ.), c/o Mrs S. Isaacs, 54, Regent's Park Road, N.W. 1.
Laycock, S. R., M.A., B.Ed., Ph.D. (Lond.), University of Saskatchewan, Saskatoon, Saskatchewan, Canada.
Gros-Clark, F., B.A. (Oxon.), 19c, Eagle Street, Holborn, W.C. 1.
, K., Old House, Ashley Heath, Hale, Cheshire.
ett, Miss J. M., 12, Victoria Crescent, Glasgow, W. 2.
s, E. O., B.A., D.Sc., M.R.C.S., L.R.C.P., 29, Highbury New Park, ighbury, N. 5.
H. R., B.A., 11, Upper Phillimore Gardens, W. 8.
iss M. G., 16, Gordon Street, W.C. 1.
Mrs E. M. F., Ruby Street (Infants) L.C.C. School, N. Camber- E. 15.
am, M.A., Mount Allison University, Sackville, New Brunswick,
, Dol-Eglwys, Builth Wells, Breconshire.
avies, Miss M. H., M.A., Fairfield, Brimstage, Wirral, nr Birkenhead.
e, Miss J., M.A., Lane End, Harpenden, Herts.
rain-Smith, Miss I., M.A., H.M. Inspector of Factories, Prudential Buildings, Grey Friars, Leicester.
Lothian, A. J. D., 9, Coates Gardens, Edinburgh.
Loudon, Rev. Alexander, M.A., B.D., The Manse, Falkirk.
Love, Wm H., M.A., Rowanbank, Craigendoran, Helensburgh, Dumbartonshire.
Low, Miss B., B.A., 13, Guilford Street, W.C. 1.
Lowenfeld, Miss M. F. J., M.R.C.S., L.R.C.P. (Eng.), 46, Queen Anne Street, W. 1.
owson, J. P., M.A., M.D., University of Queensland, Brisbane, Australia.
umsden, James, M.A., B.Ed., Moray House School, Canongate, Edinburgh.
unniss, Miss B. A., B.A., 80, Ladbroke Road, W. 11.
Macauley, Miss E., M.A. (Oxon. and Birmingham), 7, Prospect Street, Exeter, Devon.
Macbeth, Alison N., M.B., B.S. (Lond.), 39, Mecklenburgh Square, W.C. 1.

Mace, C. A., M.A., The Psychological Dept., The University, St Andrews, Fife.
MacGregor, Dr, Homerton College, Cambridge.
Mackay, R. J., B.A., 23, Bloomsbury Square, W.C. 1.
Mackenzie, Mrs., M.A. (Oxon.), Bethlem Royal Hospital, London, S.E. 1.
M.E.I. MacKenzie, Sir W. L., M.A., M.D., 14, Belgrave Place, Edinburgh.
Mackie, John, M.A., B.Sc., 19, Beresford Avenue, Leith.
Mackintosh, Miss, Inverclay, 38, Coleshill Street, Sutton Coldfield, nr Birmingham.
MacLeod, J., 177, Hill Street, Glasgow, C. 3.
MacNaughton, Mrs D. A., Forum Club, 6, Grosvenor Place, S.W. 1.
M. MacNiven, Angus, M.B., Ch.B., D.P.M., Craig House, Morningside, Edinburgh.
M.I. Macrae, Angus, 13, Willoughby Road, Hampstead, N.W. 3.
E. MacTaggart, Dr M. M., M.A., B.Ed., 333, Holly Lodge Mansions, Highgate, N. 6.
E. Makhzangi, M. H., B.A., University, Edmund St, Birmingham.
M. Manley, Miss M. M., 36, Cranley Gardens, S.W. 7.
Manning, W. E., 18, Sunnymead Road, Roehampton, S.W. 15.
M.I. Manning, W. H. O'N., M.A., 11, Carlton Mansions, Portsdown Road, W. 9.
M. Maries, Edward de, 102a, Westbourne Grove, W. 2.
E. Mark, H., St John's College, Agra, U.P., India.
M.E. Marsden, R. E., B.Sc., 45, Milton Avenue, Wellsway, Bath.
Marshlain, T. P.
Martin, O. S., M.R.C.S., L.R.C.P.
M. Matthew, D., M.B., Ch.B., Redgarth, 11, Rockfield Crescent, Dundee.
McAlexander, Mrs Ulysses G., Maxhaven, Newport, Oregon.
M. McAlpine, W., L.C.R., 5, Clifton Place, Glasgow, W.
McCarthy, Rev. Raphael C., S.J., Ph.D., St Louis University, St Mo., U.S.A.
E. McClelland, Wm, M.A., B.Sc., B.Ed., 16, Douglers Terrace, Brough
M. McCurdy, Dr J. T., Corpus Christi College, Cambridge.
E. McDonald, Miss E. (Sister Elizabeth), Notre Dame Seconda 17, Victoria Circus, Dowanhill, Glasgow.
M.E. McFarlane, Miss M., Ph.D., 23b, North Hill, Highgate, N. 6.
Ae. McGowan, H., 12, Allerton Road, Southport.
McKenzie, Rev. J., M.A.. Wilson College, Bombay.
Mears, W. G. A., M.A., High School, Germiston, South Af
Melson, G. H., M.D., 102, Alcester Road, Moseley, Birmi
Mendis, R. J. F.
M. Menon, M. P. Kesava, M.B., B.Ch., St Winifred's, Edmonton, N. 9.
Mercier, Miss Winifred, Whiteland's College, Chelsea,
Messenger, Rev. Ernest, North Lodge, Poles, Ware, Herts
Messer, Andrew, M.B., B.Sc., Medical Officer of Health, Lea Tyne, Northumberland.
E. Messer, W. J., M.Sc., 135, New Road, Chingford, E. 4.
E. Michael, Miss M., B.A., LL.B., 27, Sandringham Road, Golders Green,
E.I. Miles, George H., D.Sc., Burlington, Richmond Road East, N Barnet.
E. Miller, Miss E., 31, Pemberton Gardens, N. 19.
M. Miller, E., M.A., M.R.C.S., L.R.C.P., 28, Wimpole Street, W. 1.
M. Miller, Prof. E. M., University of Tasmania, Hobart, Tasmania.
M.E Miller, H. C., M.A., M.D., 146, Harley Street, W. 1.
M. Mills, Prof A. E., c/o Librarian, Fisher Library, University of Sydney, Australia
Minty, Miss A., B.A., St George's Court, Bourneville. Birmingham.
M. Mitchell, T. W., M.D., Hope Cottage, Hadlow, Kent.
M. Money-Kyrle, R., Ph.D. B.A. (Camb.), Whetham, Calne, Wilts.
Morgan, G. D., Maydene, Alexandra Road, Farnborough, Hants.

Morgan, Prof. G. Lloyd, LL.D., F.R.S.. 79, Pevensey Road, St Leonards-on-Sea.
E. Mostyn, Capt. J., M.A., Deerslea, Broadway, Llandrindod Wells.
E. Mowat, Geo., M.A., B.Ed., Ed. Dept., Moray House, Edinburgh.
M.E. Moxon, C., M.A., 490, Post Street, San Francisco, California, U.S.A.
Muir, A., M.A., B.Sc., Ph.D., 18, Sydney Terrace, Edinburgh.
E. Munday, Miss Rose E., Darenth Colony Schools, Dartford, Kent.
Munro, M. S., M.A., B.Ed., National Inst. Ind. Psychology, Aldwych House, W.C. 2.
M. Murray-Lyon, T. M., M.D., 46, Palmerston Place, Edinburgh.
M. Murti, Capt. G. S., I.M.S., Lattice Bridge Road, Adyar, Madras, S., S. India.
Muscio, Mrs B., Neots Avenue, Potts Point, Sydney, Australia.
M.E.I.Ae. Myers, C. S., M.D., Sc.D., F.R.S., 5k, Montagu Mansions, W. 1.
Neilson, R. M., Whit. Exhib. M.I.Mech.E., Atlantic Chambers, 45, Hope Street, Glasgow, C. 2.
E. Nevill, Miss E. Mildred, Oakington, The Ridgeway, Enfield.
E. Newby, Miss C. R., B.A., S.Th., St Christopher's College, Blackheath, S.E.
E. Newcomb, Miss E. J., 1, Grosvenor Road, Hyde Park, Leeds.
M.Ae. Newcombe, Miss M., B.A., 52, Northway, N.W. 11.
M. Nicoll, Maurice, M.D., 146, Harley Street, W. 1.
M. Nixon, J. C., M.B., Plympton House, Plympton, S. Devon.
I. Noble, Ralph A., M.B., Ch.M., Harley, 143, Macquarie Street, Sydney, N. S. Wales, Australia.
Norman, Mrs E., Bruton House, Mortimer West, Reading.
Norton, Flight-Lieut. F. A., M.Sc. (Cantab.), A.M.I.Mech.E., M.I.Ae.E., R.A.F., c/o Air Ministry, Kingsway, W.C.
Nunn, Prof. T. P., M.A., D.Sc., London Day Training College, Southampton Row, W.C. 1.
Oakley, C. A., B.Sc., Ed.B., 99, Fellowes Road, Hampstead, N.W. 3.
Oates, D. W., M.A., F.C.P., Aelybryn, 13, Dewsland Park Road, Newport, Mon.
Connor, Miss N. M., M.A. (Oxon.), 11, Cavendish Square, W. 1.
Donnel, S. P., I.C.S.
en, C. K., M.A., 10, King's Parade, Cambridge.
am, Mrs, 16, St James's Square, Bath.
y, Rev. Arnold H., M.A., The Presbytery, 3, Collington Street,
ston, Notts.
ohn, M.A., Training College, Caerleon, Mon.
J. H. Parker-, B.Sc., Wattville Road Senior School, Handsworth,
ngham.
rs Mary, Lobswood, South Hill Avenue, Harrow.
Lt.-Col., Lobswood, South Hill Avenue, Harrow.
thorpe, Dr Grace, M.B., B.S.Durham, 71, Harley Street, W. 1.
lmer, H. M., B.A., M.R.C.S., L.R.C.P., Swaylands, Penshurst, Kent.
almés, Rev. Fernando M., S.J., Ph.D., D D., Prof. in Psychology, Laboratorio Psicologico del Collegio Maximo de S. Ignacio, Sarriá, Barcelona, Spain.
.I. Parsons, J. H., F.R.S., D.Sc., F.R.C.S., 54, Queen Anne Street, W. 1.
Paterson, Miss A. T., M.A., B.Ed., 10, Crown Circus, Glasgow.
Pattullo, Dr Constance Isobel, 144, Harley Street, W. 1.
Payne, Dr Sylvia, M.B., B.S. (Lond.), 143, Harley Street, W. 1.
Pear, Prof. T. H., B.Sc. 18, Chatham Grove, Withington, Manchester.
Pearson, F. J., M.Sc., Briarfields, Theydon Bois, Essex.
Penrose, L. S., M.A., Oxhey Grange, Watford, Herts.
Perkins, Rev. F. H., 39, College Road, Moseley.
Peyton, Miss E., Littleton House School, Girton, Cambridge.
Phelan, Rev. Dr G. B., St Michael's College, Toronto, 5, Canada.
Phillips, G. E., M.A., D.Sc., The Special School, Glenfield, N.S.W., Australia.
Phillips, H., 141, Stockwell Park Road, S.W. 9.
Phillips, Miss M., The Hostel, County Training College, Crewe.

Philp, Rev. Howard L., B.A., 77, St Julian's Farm Road, West Norwood S.E. 27.
Ae. Philpott, Miss A. J., 19, Ostade Road, Upper Tulse Hill, S.W. 2.
E. Philpott, S. J. F., B.Sc., Wick Drive, Wickford, Essex.
E.I. Pickford, G. H., B.A., Holly Bank, 195, Dudley Hill Road, Bradford.
Pickford, R. A., M.A., Dept. of Psychology, The University, Glasgow.
Pitt-Rivers, Capt. G., Manor House, Hinton St Mary, Dorset.
E. Pitts, Miss M., B.A., 16, Palmerston Road, N. 22.
E. Polishuk, Mrs P. L., 21, Torrington Park, N. Finchley, N. 12.
E. Porter, T. L. D., B.A., B.Sc., 162, Coventry Road, Ilford.
Pottinger, Rev. W., M.A., 21, Broompark Drive, Dennistoun, Glasgow, E. 1.
M. Potts, Dr W. A., 118, Hagley Road, Edgbaston, Birmingham.
Pragasam, A. G., B.A., 1, Blake Road, Colombo, Ceylon.
E. Preddy, Miss, 38, Martell Road, E. Dulwich, S.E. 21.
M. Pride, Miss L. E., 12, Bentinck Terrace, N.W. 8.
E. Punnett, Miss, London Day Training College, Southampton Row, W.C. 1.
Purushottam, T. A., c/o Psychological Laboratory, University College, W.C. 1.
Quance, F. M., Ph.D., University of Saskatchewan, Saskatoon, Saskatchewan, Canada.
M. Radclyffe, E. J. D., M.A., 37, Gt Ormonde Street, W.C. 1.
M. Rae, J. Burnett, M.D., Ch.B., 82, Harley Street, W. 1.
Ramsey, Mrs L. C., M.A., Hoop Chambers, Bridge Street, Cambridge.
Rankin, Miss Mabel I., M.A., B.Ed., B.Sc., 1794, Cumbernauld Road, Mil ston, Glasgow.
E.I. Raphael, Mrs W., B.Sc., 6, Allen Mansions, Allen Street, W. 8.
E. Rawson, Miss H. L. C., B.A., 105, Headingley Mount, Leeds.
Read, A. C., M.A., 8, Joubert Mansions, King's Road, Chelsea.
M.E.I. Read, Prof. C., Woodlane, Birmingham Road, Solihull, Warwickshi
M. Read, C. S., M.D., 71, Harley Street, W. 1.
M.I.E. Reckitt, Miss E. C., 9, Old Square, Lincoln's Inn, W.C. 2.
M. Rees, J. R., M.A., M.D., 14, Wimpole Street, W. 1.
M.E.I. Reeve, L. R., 11, Crescent Way, Brockley, S.E. 4.
M. Reynell, W. R., M.D., M.R.C.P., 87, Harley Street, W. 1.
M.E. Richards, Miss G. A., 6, Greycoat Gardens, Westminster, S.
E. Richardson, Dr Lewis Fry, F.R.S., F.Inst.P., Dunsc Paisley, Scotland.
M. Rickman, John, M.A., M.B., B.Ch., 37, Devonshire Place
M. Riddoch, G., M.D., 16, Devonshire Place, W. 1.
M. Riggall, R. M., M.R.C.S., L.R.C.P., 31, Wimpole Street
M. Riviere, Mrs J., 3, Stanhope Terrace, W. 2.
I. Roberts, Miss G. H., B.Sc., 1, Wilton Terrace, S.W. 1.
Ae. Roberts, Mrs Ursula, B.A., 19, Woburn Square, W.C. 1.
M. Robertson, A., M.B. (Edin.)
M Robertson, Prof. G. M., M.D., Tipperlinn House, Morningside Place, burgh.
Robertson, J. Boyd,
M. Robinson, Prof. A., M.A., D.C.L., Observatory House, Durham.
I. Robinson, Miss D. F., 2, Gloucester Crescent, Regent's Park, N.W. 1.
Robinson, Miss M. E., 1, Holmbury View, Upper Clapton, E. 5.
Robinson, W., M.B., D.P.M., Ch.B., City of London Mental Hospi Dartford, Kent.
I. Rodger, T. A., B.A., 47, Gt Ormonde Street, W.C.
E. Roker, Peter, M.Sc., 122, Stamford Street, S.E. 1.
M.E. Ross, Miss E. L. S., M.A., B.Ed., Education Office, Council House, M Street, Birmingham.
M.E.I. Ross, G. R. T., M.A., D.Phil., c/o Overseas Club, Park Place, St Ja S.W. 1.

E. Ross, James S., M.A., B.Sc., Westminster College, 130 Horseferry Road, Westminster, S.W. 1.
M. Ross, T. A., M.D., M.R.C.P.E., The Cottage, Swaylands, Penshurst, Kent.
I. Rowntree, B. S., J.P., The Technical Library, Messrs Rowntree & Co., Ltd., York.
M.E.I.Ae. Rusk, R. R., M.A., Ph.D., 14, Crown Gdns., Glasgow, W.
Rusk, Rev. W. M. R., M.A., B.D., Ph.D., Hope Park Manse, 25, Fountainhall Road, Edinburgh.
E. Russell, J. B., B.Sc., Southend House, Bury St Edmunds, Suffolk.
E. Sahai, Mahajot, M. L., 104, Charing Cross Road, W.C. 2.
Said, M. Mazhar, M.Sc., University, Edmund Street, Birmingham.
Saidullah, M.A., Ph.D., Kucha M. Asadullah Vakil, Amritsar, India.
M. Salter, Miss H. de G., The Crown House, Newport, Essex.
Samuel, Miss E. A., M.A., B.Ed., 43, East Trinity Road, Edinburgh.
E. Sandon, Frank, M.A., 5, Huddleston Road, Tufnell Park, N. 7.
E. Sawdon, E. W., M.A., B.Sc., Kings Mead, Selly Oak, Birmingham.
Saxby, Miss I. B., D.Sc., 21, Y Groes, Rhubina, Cardiff.
M. Saywell, Mrs Evelyn Neville, L.R.C.P. & S. (Ed.), 6, Albany Terrace, N.W. 1.
Scales, W. T., 32, Kelvin Road, Highbury Park, N. 5.
Schiller, F. C. S., M.A., D.Sc., Corpus Christi College, Oxford.
.E. Schiller, L. C., B.A., 36, Bentley Road, Sefton Park, Liverpool.
Schonell, F. J., B.A., 36, Harvey Road, Hornsey, N. 8.
Scoresby-Jackson, Miss M., M.D., B.S., 86, Harley Street, W. 1.
Scott, Miss E., B.A., Education Dept., The University, Edmund Street, Birmingham.
Scott-Maxwell, J. M., B.Sc., M.I.E.E., Branscroft, Rookfield, Renfrewshire.
Searl, Miss M. N., 16, Gordon Street, W.C. 1.
Sedgwick, Russell, 3, Clifton Hill Studios, N.W. 8.
Seligman, Prof. C. G., M.D., F.R.S., Court Leys, Toot Baldon, Oxford.
elwood, Ernest H., M.A., B.Sc., 2, Grove Park Avenue, E. 4.
n, Mrs H., 112, Gower Street, W.C. 1.
nett, Miss M., 90a, Fielding Road, Bedford Park, W. 4.
eant, Miss F. I., The Training College, Derby.
ur, A. H., B.Sc., National Institute of Industrial Psychology, Aldwych
use, W.C. 2.
A. F., M.A., 1, Edwardes Place, Kensington, W. 8.
iss E., 16, Gordon Street, W.C. 1.
rs M. I., M.Sc., 6, Addison House, Grove End Road, St John's
N.W. 8.
4, Whitehall Court, S.W. 1.
Dare, Miss H. M., B.A., 39b Linden Gardens, W. 2.
rkar, D. D., B.A., B.T., 5, Corennie Road, Tufnell Park, N. 19.
ppard, Miss Emily A., 62, Chudleigh Road, Brockley, S.E. 4.
eppard, Dr W. F., Cadrona, Berkhamstead.
Sherrington, Prof. Sir C. S., M.D., P.R.S., The Physiological Laboratory, Oxford.
Shine, Rev. Prof., M.A., 24, St Kevin's Park, Dartry Road, Dublin, Ireland.
Shivaprakasam, K., 84, Boundary Road, N.W. 8.
E. Shrubsall, F. C., M.D., 15, Well Walk, Hampstead, N.W. 3.
Simmins, Miss C. A., 19, Woburn Square, W.C. 1.
Simpson, Mrs L. E., B.A., Education Dept., University, Edmund Street, Birmingham.
Skrimshire, Miss L. D., 55, Hanover Road, Willesden, N.W. 10.
Slight, D., M.B., Ch.B., McGill University, Montreal, Canada.
Slocombe, C. S., B.Sc., Ph.D., Boston Elevated Railway, York Square Buildings, Boston, U.S.A.
Small, Miss C. W., M.A., 50, Roslea Drive, Dennistoun, Glasgow.
Smith, F., M.A., 32, Swan Lane, Bolton.

E. Smith, Prof. F., University College, Newcastle.
E. Smith, G. E., 201, Stanley Park Road, Carshalton, Surrey.
M. Smith, G W., M.B , Ch.B., Wyke House, Isleworth, Middlesex.
Smith, J. W. D., M.A., 27, Herriet Street, Glasgow.
M.I. Smith, Miss M., 37, Abbey Gardens, St John's Wood, N.W. 8.
Smith, M. H., M.A., M.D., H.M. Prison, Birmingham.
E. Smith. Miss P. Woodham, M A , Maria Grey Training College, Brondesbury, N.W. 6.
E. Smith, Rev. T. Grigg, M.A., Warley Vicarage, Halifax.
• Snow, A. J., 2020, Sherman Avenue, Evanston, Illinois, U.S.A.
M. Snowden, E. N., M.B., M.R.C.S., L.R.C.P., 21, New Cavendish Street, W. 1.
E. Sparling, Miss L. C., Municipal Training College, Cottingham Road, Hull.
M.E.I. Spearman, Prof. C., Ph.D., LL.D., F.R.S., 71, Kensington Gardens Square, W. 2.
E. Spence, Miss N.
M. Sprott, W. J. H., B A., 29a, Clumber Street, Nottingham.
E.I. Stanford, R. J., M.A., National Institute of Industrial Psychology, Aldwych House, W.C. 2.
M. Statham, H., M.B., B.Ch., M.R.C.S., L.R.C P., 31, Christchurch Road, Bournemouth.
Ae. Stebbing, Miss L. S., M.A., 27, Belsize Park, N.W. 3.
M.E. Stephen, A. L., 50, Gordon Square, W.C. 1.
Stephenson, Arthur, B.Sc., New Modderfontein Gold Mining Co. Ltd., Mi Office, P.O. van Ryn, Transvaal, Africa.
M.E.I.Ae. Stoddart, W. H. B., M.D., F.R.C.P., Harcourt House, Cavendish Squ W. 1.
M. Stoddart, Mrs W. H. B., Harcourt House, Cavendish Square, W. 1.
Stokoe, Miss Betty, 61, Westbourne Terrace, W. 2.
I. Stott, Miss Mary B., B.A., 40, Priory Gardens, Highgate, N. 6.
Stout, Prof. G. F., LL.D., F.B.A., Craigard, St Andrews, Fife.
E. Stow, Miss Lucy, Training College, Ripon.
M. Strachey, James, B.A., 41, Gordon Square, W.C. 1.
M. Strauss, E. B., B.M., B.Ch., B.A. (Oxon.), M.R.C.P. (Lond.), Street, W. 1.
E. Streeten, Mrs C. F.
M.E. Streeter, Rev. Canon, M.A., D.D., Queen's College, Oxfor
E. Studman, Miss L. G.,13, Upper Park Road, Hampstead, N
Sturt, Miss M., B.A., 14, Poulton Square, Chelsea, S.W.
Sutherland, Capt. H. E. G., M.A., B.Ed., 31 I Portobello, Edinburgh.
Sutherland, J. D., B.Sc., B.Ed., 16, Dalziel Place, Edin
M. Suttie, I. D., M.B., B.Ch., F.R.S.P.S , Kenlaw House, Coli
M. Tansley, Prof. A. G., M.A., F.R.S., Grove Cottage, Grantchester
M.E.Ae. Taylor, Miss N. G. R., Avery Hill College, Eltham, S.E. 9.
I. Taylor, S. E. W., M Sc., Toynbee Hall. Commercial Street, E. 1.
M.E Terry, Miss Ella M., 20, Manchester Street, Manchester Square, W. 1.
M.E. Thomas, Miss B., 9, Lansdowne Crescent, Cheltenham.
E. Thomas, F. C., B.Sc., 58, Birley Road, Whetstone, N. 20.
Thomas, Dr G., 382, Bearwood Road, Smethwick, Birmingham.
Thomas, Rev. J. M. Lloyd, 47, Frederick Road, Edgbaston, Birmingh
M.E.I. Thomas, Miss Maud Tuson, 2, Brondesbury Park, N.W. 6.
M. Thomas, Dr Rees, Rampton State Institute, Retford.
E. Thomson, Miss Edith, M.A., B Ed., 145, Dalkeith Road, Edinburgh
E.I. Thomson, Prof. G. H., D.Sc., Ph.D.. The University, Edinburgh.
M. Thomson, H. T., M.D., 13, Lansdowne Crescent, Edinburgh.
E. Thomson, Miss J. R., M.A., The Training College, Derby.
Thomson, R. G. 5, Lincoln House, Dartmouth Park Hill, N.W. 5.
E. Thompson, J. R., D.Sc., Education Dept., University, Sheffield.

Thorburn, J. M., University College, Cathay's Park, Cardiff.
Thorp, Sidney Benson, B.A., 44, Claygate Road, West Ealing, W. 13.
M. Thouless, R. H., M.A., Ph.D., Psychological Laboratory, University, Glasgow.
E. Tibbey, T. G., M.A., 16, Tyson Road, Forest Hill, S.E. 23.
M. Tippet, J. A., M.R.C.S., L.R.C.P.
M. Tolputt, A., M.D., 93, The Headlands, Kettering.
E. Tompkins, A. G., Hutcombe Hotel, Taunton, Somerset.
E. Torrance, W. G., B.A. (Lond.), Holmewood, Uttoxeter, Stafford.
M. Townsend, Miss St Clair, North House, Northwood, Middlesex.
E. Tregear, W. T., B.A., 29, Anglesea Road, Ipswich.
M. Tremellen, Dr, 7, Buckingham Palace Gardens, S.W. 1.
Trimble, Dr Otis C., M.S., Ph.D., Dept. of Education, Bradley Polytechnic Inst., Peoria, Illinois, U.S.A.
M. Turnbull, Peter Mortimer, M.B., Mental Hospital, Caterham, Surrey.
M.E. Turner, Miss J., B.A., 14, Endsleigh Street, W.C. 1.
M. Turton, Miss W., 18, Hanover House, Regent's Park, N.W. 1.
E. Twentyman, A. G., 6, Stanhope Gardens, Highgate, N. 6.
M. Tydeman, Miss M. M., B.A., 60, King's Road, Wimbledon, S.W. 19.
Tyrrell, Miss B. D., 170, Holland Road, W. 14.
Ae. Urwick, Major L., M.A. (Oxon.), Les Mayens, Petit Saconnex, Geneva, Switzerland.
E. Valentine, Prof. C. W., M.A., D.Phil., Greenland House, Greenland Road, Selly Park, Birmingham.
Vance, Rev. Dr J. G., M.A., Ph.D., Little Priory, Kimpton, Herts.
Vaughan, Mrs C., Courtfield, Ross, Herefordshire.
Vernon, H. M., M.A., M.D., 5, Park Town, Oxford.
Vernon, Miss M. D., M.A., Psychological Laboratory, Cambridge.
Vickers, Wilfred, M.Sc., M.Ed., Orchardlea, Ashford Road, Bearsted, near Maidstone.
Wace, Richard Henry, M.B., C.M. (Aberd.), Millbrook, Milford Lane, Highgate, N. 6.
Ait, Miss M. H., 48, Featherstone Street, Roker, Sunderland.
lker, N. T., M.A., Ed.B., Ph.D., Dept. of Education, University, Aberdeen.
as, Prof. G., M.A., 38, St Leonard's Terrace, Chelsea, S.W. 3.
rs, Miss Elsa H., B.A., 26, Clonmel Road, Teddington, Middlesex.
all, Miss E. A., Ashgrove, Trinity Road, Darlington.
Prof. S. H., M.A., Phil.D., University College, Exeter.
pt. A. E., M.A., H.Q. 3rd Division, Bulford Camp, Wilts.
D.Sc., Berwyn, Totteridge, N.
H. M., 6, Taviton Street, Gordon Square, W.C. 1.
. J., B.A., The College, Caerleon, Mon.
Jack, B.Sc., 135, Clapham Road, S.W. 9.
h, H. J., 1–4, Great Tower Street, E.C. 3.
st, R. P. H., B.A., The Central School, Rosettenville, Johannesburg, South Africa.
Wharton, Miss E. A., M.R.C.S., L.R.C.P., Withyham, Sussex.
Wheeler, Miss E. C., B.A., 7, Holborn House, Baldwin's Gardens, Grays Inn Road, W.C.
E.I. Wheeler, Prof. O., D.Sc., University College, Cathays Park, Cardiff.
Whipple, R. S., M.I.E.E., 13, Holly Lodge Gardens, West Hill, Highgate, N. 6.
White, H. D. Jennings, M.A., Ph.D., 41, Gower Street, W.C. 1.
White, Mrs J., D.Sc., Auto-Education Institute, 46, Great Russell Street, W.C. 1
Whiting, G. E. T., B.Sc., 4, Valley Road, Erith, Kent.
Wilkes, G. A., M.D., 120, Trinity Road, Handsworth, Birmingham.
Wilkinson, Miss E., Neuadd Went, Training College, Barry, Wales.
Williams, A. C., D.P.H., M.R.C.S., Crindledyke, 4, Coombe Road, Croydon.
Williams, Miss Beryl, Tamjmarian, 34, Trinity Road, Llanelly.

Williams, Rev. C. V., M.A. (Cantab), Auchingramont Manse, Hamilton, Lanarkshire.
Williams, Lady, 47, Eaton Place, S.W. 1.
M. Wilson, A. C., M.R.C.S., L.R.C.P., 27, Nottingham Place, W. 1.
I. Wilson, D. R., M.A., H.M. Deputy Chief Inspector of Factories, Home Office, S.W. 1.
M. Wilson, E. A., M.D., 10, Bloomsbury Square, W.C.
Wilson, J. H., M.Ed., M.Sc., 97, Springfield Road, Brighton.
M. Wiltshire, J., M.R.C.S., L.R.C.P., Wayside, Princes Risborough, Bucks.
• Wimms, J. H., M.A., B.Sc., Goldsmith's College, New Cross, S.E. 14.
E. Wisdom, Alfred, B.A., B.Sc., London, 66, Ellesmere Road, Chiswick, W. 4.
M.E.I. Wohlgemuth, A., D.Sc., Rutland Lodge, May's Hill Road, Shortlands, Kent.
Wolf, Prof. A., M.A., D.Litt., 57, Thurleigh Road, Wandsworth Common, S.W. 12.
M.E. Wolters, A. W., M.A., Reading University, Reading.
E. Wood, Miss M. H., The Cambridge Training College for Women, Wollaston Road, Cambridge.
Woodhull, Miss Z. M., Narton Park, nr Tewkesbury, Worcestershire.
M.E. Woods, Miss A., St Ives, Cross Path, Radlett, Herts.
Ae. Wright, Miss E., 45, Highbury New Park, N. 5.
E. Wright, G. G. N., M.A., B.Ed., Dept. of Adult Education, University Colleg Nottingham.
M. Wright, M., M.D., 86, Brook Street. Grosvenor Square, W. 1.
Wyatt, S., M.Ed., Wycroft, Eccles Road, Chapel-en-le-Frith, Derbyshire
E.I. Wynn-Jones, Ll., M.A., Ph.D., Psychological Department, University Leeds
M. Yates, Mrs, M.R.C.S., L.R.C.P., 16, Tufnell Park Road, N. 7.
M. Yellowlees, D., M.B., Ch.B., 5, St James's Terrace, Glasgow, W.
E. Young, Miss M., M.A., B.Ed., c/o Mrs Fergus, 19, Crawfurd Road, Edin
Yule, Miss Ella Pratt, Gowan Park, Cupar, Fife, Scotland.
E. Yule, G. U., M.A., F.R.S., St John's College, Cambridge.

ASSOCIATE MEMBERS.

I. Finch, Miss D. B. U., c/o Shoolbred & Co., Ltd., Tottenham Cou
I. Hornsby, L. H. c/o Shoolbred & Co., Ltd., Tottenham Cour
I. Newcomb, Miss E. D., St Ives, East Grinstead.

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ancement of Psychology shall be eligible for election as Honorary
bers.

Honorary Members shall be entitled to all rights and privileges of
rship.

onorary Members may be elected from time to time on the
of the Council at any General Meeting of the Society.

ouncil shall be empowered to institute Sections of the Society,
being concerned with a special branch or aspect of Psy-

a Section or Sections shall *ipso facto* constitute election
whole and entitle those so elected to receive notice
in all General Meetings of the Society in addition to
the Section or Sections to which they belong.

tions shall have the right of electing Sectional Associates who
eive notice of and shall be permitted to attend all Meetings of
Section or Sections to which they belong. Such Sectional Associates
all however take no part in the Government of the Society or the
ions, and they shall not receive *The British Journal of Psychology.*

a). Transfer from Sectional Membership to Sectional Associate-
y take place on January 1st only, and after not less than six
notice from the Member of his wish to transfer.

The procedure as regards the election of Sectional Associates
the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot to take place can vote by sending his balloting paper to the Secretary

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to Candidate's name; to vote *against* a Candidate, he must erase Candidate's name. Members who wish to record a vote neither f against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for t may, after receiving due notice, be removed by a resolut Council from membership of the Society.

19. Whenever it shall be proposed to remove a Me Society for any reason other than failure in payment of s matter shall be brought as a formal resolution before t of the Sectional Committees (whichever it shall most event of the resolution being passed, a ballot shall be he of the said Member at the next General Meeting of the So next meeting of the Section respectively. Should two-thi Members voting vote in favour of the removal of the Memb Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, t subdivide any Section or Sections, or to amalgamate two or Sections, a Special Meeting of the Section or Sections concerned called for this purpose according to the conditions laid down in R Resolutions passed at any such Meeting shall be reported by the S Secretary or Secretaries concerned to the next Meeting of the C Should the Council approve of such resolutions, such resolution immediately take effect. But should the Council formally express

approval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a
Council consisting of the President, Vice-Presidents, Treasurer, General
ecretary, Librarian, Editors of *The British Journal of Psychology*, one
presentative each of every Section and Branch to be nominated by the
tional and Branch Committees, one representative of the Committee
Research in Education to be nominated by that Committee, and of
rdinary Members.

The President of the Society shall hold office for three years and
ot be eligible for re-election until after the lapse of a further
f six years. Every President shall at the termination of his
office become a Vice-President for a period of six years. The
s of the Society shall retire annually but shall be eligible for

ear two ordinary members of the Council, having served
shall retire and shall be ineligible for re-election until
one year.

Members to serve on the Council may be suggested by
by any Member by sending the proposed names to the
at least two weeks before the Annual General Meeting.

Nominations for the election of Officers shall be handed in to the
eneral Secretary before the Meeting preceding the Annual General
Meeting and be announced at the Meeting preceding the Annual General
ting.

One full week before the Annual General Meeting, the General
ary shall send to each Member a balloting list containing: (1) The
of the Council; (2) The number of the attendance of members of
uncil at the Council Meetings held during the year; (3) The names
ose proposed by the Council or by Ordinary Members to fill the
ancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the ordinary members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section sha appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annua but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by Section. A quorum at a Meeting of a Sectional Committee shall that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and pressly stated to the contrary) apply respectively to the C Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, di in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE

34. Each Ordinary Member shall pay an annual s guinea, which shall entitle him to receive a copy of *The of Psychology*, with the exception of such additional portio *Journal* as are devoted to special branches or aspects of Psycholog are published separately.

34 (*a*). Ordinary Members who are members of all Sections sha pay an inclusive annual subscription of £2. 12*s*.

35. In addition to the Ordinary Membership Subscription M shall pay an additional fee for every Section which they joi additional fee shall entitle Members of any Section to receiv additional, and separately published portions of *The British Jou Psychology* as may be devoted to the special branch or aspect o

chology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall
ntitle the Member to all the privileges of membership, which he enjoys
t the time of composition, for life.

38. The subscription is payable in advance and becomes due on
nuary 1st. *The British Journal of Psychology* shall be sent only to
bers whose subscriptions have been duly paid.

9. The Council shall grant to each Section permission to incur such
ses as may be necessary for the work of the Section; the approxi-
mount of such expenses to be determined by the Council.

MEETINGS.

e Annual General Meeting shall be held in December, when the
retary's Report and the Treasurer's Statement of Accounts
efore the Society.

eral Meetings of the Society shall be at such times and
ncil shall decide, but in each year there shall be at least

sident, or, failing him, a Vice-President, shall, whenever
as Chairman at the Meetings of the Council and of the
n the absence of the President or a Vice-President, some other
er shall, on the motion of the General Secretary or his substitute,
elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices
ch Meeting at least seven days beforehand, and of the business to
sacted at the Meeting.

An Extraordinary General Meeting may be summoned at any
the Council, or by the General Secretary on the written request
ty Members; at least fourteen days' notice of such Meeting shall
t to each Member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve th
establishment of Branches of the Society throughout the British Empir
Members of such Branches shall pay the same subscription as that
Ordinary Members direct to the Treasurer of the Society and be enti
to the same rights and subject to the same rules as Ordinary Mem
The Council shall be empowered to refund to the Treasurer of any B
such proportion of Branch Members' subscriptions as the Counc
determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be
mitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to fo
shall be 10.

46 *c*. Branches shall be required to present a report
General Meeting.

RULES.

47. The Rules of the Society may be altered at the A
Meeting, after notice of alteration has been given at a previou
General Meeting, or they may be altered at an Extraordinary
Meeting summoned for that purpose. Such Extraordinary Gene
Meeting shall be held in London, and the purpose for which it is calle
shall be expressly stated in the notice convening the Meeting. No o
business shall be transacted thereat.

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VOLUME XXI OCTOBER, 1930 PART 2

THE PSYCHOLOGY OF IMITATION WITH SPECIAL REFERENCE TO EARLY CHILDHOOD

BY C. W. VALENTINE.

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II. *Earliest appearance of imitation: sound making at one month* (pp. 107–109).
III. *Second month: smile responses* (p. 109).
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I. WIDE DISAGREEMENT AS TO PSYCHOLOGY OF IMITATION.

THE psychology of imitation is in a somewhat chaotic state, partly owing to the ambiguity of the term and partly owing to disagreement as to facts.

Thorndike[1] quotes Kirkpatrick, Stout and others as examples of upholders of a widespread view that "the child makes every gesture that he sees and every sound that he hears": and adds that any who seem to have said that "behaviour witnessed produces identical behaviour by any original potency" "cannot, if possessed of any sense for fact, have meant what they said."

"Sit before an infant," writes Thorndike, "and perform time after time a score of such novel but simple acts as putting your right hand on your head and your left on your right shoulder. He does not in nine cases out of ten do anything more like the act you perform than like any other of the twenty."

En revanche, Drever says: "So much of our own learning is done by way of imitation, so much of the untutored teaching of parents, civilized and savage alike, is based on imitation, so much of the teaching and training of their young on the part of the lower animals rests on the same basis, that it is difficult to believe that Thorndike means to deny that there is an instinctive tendency to imitate, in the sense in which this has always been assumed. If he does, we cannot help regarding his view as an almost incredibly arbitrary distortion, and even flouting, of the facts, in order to support a theory which has not even simplicity to recommend it[1]."

---

[1] *Educational Psychology*, I, 110.

Again, J. B. Watson says the analysis of the instinct of imitation has been only 'very superficial,' and quotes Thorndike's treatment as being 'as accurate as any[2]'; on the other hand, J. M. Baldwin describes imitation as the 'controlling impulse' when it does develop[3].

McDougall denies the existence of an instinct of imitation on the grounds that there is no 'common affective state,' no common impulse seeking satisfaction in some particular change of state[4]; that we lac[illegible] evidence of it in lower animals, and that cases of imitation can [illegible] explained in other ways[5]. Thus many cases of imitation can be explained as the result of the gregarious instinct, others can be traced to a general tendency for 'all motor representations' to realize themselves 'immediately in movement, an admission which it is surprising has not been seized upon by upholders of a 'general imitative tendency,' as giving all they asked; for they need not necessarily tie themselves down to an 'instinct' in McDougall's sense as implying strong conative urge and a specific emotion.

It is well, however, that they did not claim the authority of McDougall in their support, for in his *Outlines* McDougall shows a much more critical attitude towards this ideo-motor theory, regarding it as "an exaggeration and distortion of the truth that every cycle of mental activity tends naturally to express itself in bodily movement[6]."

More recently Koffka has stated that animals and children only imitate 'what they understand[7],' a more sweeping assertion than that of Kohler, who is careful to say that "relatively *complex* proceedings seen *without a trace of insight* are not imitated[8]."

Those psychologists I have referred to so far are, I think, fairly representative of various views in imitation. All would agree that there is such a thing as *purposive* imitation, when an individual (A.) sees another (B.) secure, by a certain method, an effect which A. also desires, and when A. proceeds to imitate the method in order also to bring about his own purpose. This is simply imitation of a means, which is comprehended as a means, to an already desired end: 'reflective imitation' I should prefer to call it (with Lloyd Morgan) in order at this stage not to exclude all purpose (in any sense of the word) from other forms of imitation.

---

[1] *Psychology of Education*, p. 87.

[2] *Psychology from the Standpoint of a Behaviourist*, p. 280.

[3] *Mental Development in the Child and the Race*, p. 125.

[4] *Social Psychology*, 9th edition, p. 103.

[5] *Outlines of Psychology*, pp. 173 ff.

[6] *Ibid.*, p. 290.

[7] *The Growth of the Mind* (Eng. trans. by R. M. Ogden), p. 318. Italics mine.

[8] *The Mentality of Apes* (Eng. trans. by E. Winter), p. 220.

It is in reference to the existence of a more non-reflective 'impulse' or 'instinctive' tendency to imitate (*primary* imitation I propose to call it) that differences of opinion arise—whether there is a general tendency, or specific tendencies, and if so, of what nature. This is obviously a question which can only be answered by a thorough study of the behaviour of children in the earliest years.

I give below the results of observations and tests extending over fifteen years on my own children, from their earliest days, quoting all my notes on anything of the type described as 'imitation' by various writers, though some might include certain of these phenomena under 'suggestion,' 'innate sympathetic response,' etc.

My boys are referred to as A., B. or C., and my daughters as X. and Y., alphabetical order not to be taken as signifying order of age. E. indicates their mother. When no letter is specified the observation refers to B.

D. stands for day, showing the age of the child referred to at the time of the observation. The observer was myself, unless otherwise stated[1].

II. EARLIEST APPEARANCE OF IMITATION: SOUND MAKING AT ONE MONTH.

My earliest note on imitation is of B. on:

D. 15. Suggestion of mouth opening in response to mine; but a baby's lips are so frequently moving that coincidence is always possible and this seems probable here as no imitation of tongue protrusion was obtained in several tests during second and third weeks with Y.

As to Y.:

D. 28. After half a minute, in response to nurse's talking Y. responded by 'talking' back, a slight 'ah' coming softly and always in intervals of nurse's talk; an obvious reply.

E. tried to get same response but was too hurried. I said "Leave intervals for Y. to reply"; and then the response came.

---

[1] As a rule exact details as to date and conditions are given, in view of their importance in the development of the general psychology of early childhood, and for the building up of criteria for tests of normal development even in the earliest months of life.

As to B. I noted also:

D. 32. Croons now in response to mine, five or six times repeated. In order to avoid the possibility of mere coincidence I used the following method. The number of his own spontaneous croons were counted during two minutes. Then during the next two minutes E. would occasionally croon to him, with this result.

		No. of croons by B.	
1st minute	No stimulus croon	8	—
2nd ,,	,, ,,	5	—
3rd ,,	E. crooning	—	12
4th ,,	,,	—	9
5th ,,	No stimulus croon	2	—
6th ,,	,, ,,	0	—

Here B. began to cry for food, so test stopped.

D. 34.	1st ½ minute	No stimulus croon	0	—
	2nd ,,	E. crooning	—	5
	(Test interrupted)			

Approximate average number of croons per minute (ignoring the 6th minute of D. 32 lest desire for food was already preventing spontaneous crooning):

No stimulus	4½
When E. crooning	10½

Here then there is a fairly clear case of one form of imitation at the age of only one month, of which other notes a few days later give further evidence.

D. 38. Guttural sound to-day, 'ar-roo,' very like French *r*, not full trill. This repeated twenty times after me: I started him with cooing and he continued so fast that counting as before seemed unnecessary labour.

D. 39. Several times recently I have started him cooing ('a-roo') when he was otherwise lying silent, and then he would go on rapidly for two or three minutes, as fast as I could reply to him, with occasional smiles.

Even earlier than this (fourth and fifth weeks) I noted several times responses made by Y. when 'talked to.'

Probably most mothers would report that they already know that such imitation takes place, but careful and measured tests are none the less necessary: for nearly every one of those mothers would also probably report with equal confidence that at three months the baby "understands everything you say to him."

A. did not at the age of one month respond to sounds nearly as much as B. did, and less than Y.[1] In similar tests with A. on D. 33 he gave on the average (over a six-minute test) 1 croon per minute when we were silent, and 2 croons per minute when E. was crooning. This difference in so short a time might, of course, be purely accidental.

---

[1] In several other ways A. revealed, even in the first two or three months, less responsiveness, activity or excitability than did B. and others: *e.g.* as to his reflexes, see the author's article "Reflexes in early childhood," *Brit. J. Med. Psychol.* 1927, VII.

Only on D. 49 did A. show very definite response to my 'arroo.' It started his cooing in response (with smiles), one coo to each of mine for six or seven times; and similarly on D. 51.

I am satisfied then that, in the three of my children in whose cases I looked for this imitation, there was definite imitation of sounds before the end of the seventh week, and in B. and Y. about the end of the first month. It is not imitation of specific sounds spoken but imitation of sound making: though one can, of course, get a similar sound made by selecting the sound 'arroo' or 'goo,' which the infant predominantly makes at the time.

The earliest other records I can find for similar imitations are as follows: as to Scupin's son at one and a half months: A brrrh imitated with considerable exertion. I noted nothing in the nature of signs of great effort at this stage, though I did later in the imitation of words.

STERN. H. 2½ months. 'Erre' repeated "with evident effort[1]."

DEARBORN. D. 59. Being gurgled to produced laughter, fairly good imitation five or six times[2].

SHINN. D. 59. Piano playing produced a singing murmur as long as music lasted[3].

III. SECOND MONTH: SMILE RESPONSES.

After the end of the second month, and especially in the fourth month, other imitations besides those of sounds appear. Before the end of the second month Y.'s smiles in response to a smile were so evident that I have notes as to myself and E. carefully avoiding smiling to see if the mere sight of a familiar face produced a smile without a smile stimulus.

D. 81. Y., first real laugh in response to my laughter.

D. 100. B., during last week or two, if not much earlier, I believe smiles have led to smiles.

D. 115. B. repeatedly laughed when his mother and I laughed to him, even when he was beginning to whimper.

This response of smiles to smiles and laughter to laughter might be held to be due to an innate tendency for a smile to cause pleasure which then expresses itself in a smile; but assuming for the time being a purely behaviourist definition of imitation, and leaving distinctions till later, it should be included under imitation.

---

[1] *The Psychology of Early Childhood*, p. 91.

[2] *Motor Sensory Development*, p. 24.

[3] *Notes on the Development of a Child*, I, p. 123.

IV. EARLY IMITATION OF OTHER MOVEMENTS.

Scupin noticed mouth opening when others were seen drinking at three months[1]. McDougall noted imitation of tongue protrusion at four months. I failed to get imitation of tongue protrusion in the second and third months with X. McDougall records this as noted at four months. With B. I tested it at five and a half months and noted:

D. 161. Signs of imitation; putting out tongue when I did, and holding it out for a time; so again later. I noticed that his eyes wandered from my eyes to my tongue continually. Similarly with A. (D. 173), "but it is very difficult to know whether it was not mere coincidence."

D. 162. On two occasions tongue put out after mine had been out perhaps half a minute; done fairly slowly, not sudden jerk (like impulsive movements), groping near mouth opening first. And often he looked at my tongue and back to eyes.

D. 181. Several times I tried to get tongue put out in imitation, but failed.

With C.:

D. 182. Got clear imitation of tongue protrusion. I did it five times and at the sixth time tongue came right to front and tip out. So at seventh and (less) at eighth or ninth. This not a habitual movement of his. Attention seemed held by my tongue after third or fourth movement.

With Y.:

At eight months. My tongue protrusion (and withdrawal) led to protrusion on her part (one to about a dozen of mine). She seemed very interested, trying to take hold of my tongue.

One note of Dearborn suggests also imitation of hand movement during these early months. Thus:

D. 87. Imitation seems to be on the way to-day, for L. immediately attempts to shake her hand when one makes this movement before her, and often with very good success. So far it is only her left hand that imitates.

Her left side was consistently more precocious than her right. I have not been able to get imitation in any other action than shaking the hand.

But this observation of any imitation of movement other than that of mouth—in speech, smiling, mouth opening, or tongue protrusion—stands alone before the age of five months: and Dearborn failed to obtain it again on the ninetieth day. His next reference to imitation is on D. 171, when imitative mouth movements are referred to.

V. IMITATION OF THE SOUND OF DEFINITE WORDS.

At six months imitation of other movements, *e.g.* arm waving, begins to appear but only very sporadically. Before very clear imitation of outer movements is observable we get more definite advance in sound imitation, namely the attempt to imitate a particular sound or word, and not the mere making of any sound in response to sound, a refinement of immense importance in advancing speech development. Thus in the case of Y. at five and a half months:

---

[1] Stern, *op. cit.* p. 91.

D. 165. When E. was coughing, Y. gave a little cough. E. repeated it several times. I heard it three or four times. (Clearly not mere laughter.) The imitation followed slowly after an interval of some seconds, sounding like a deliberate effort.

D. 197. I looked at her (without smiling) and began to make 'ph' explosive lip sounds: after a few Y. began to imitate, her part increasing as we went on, I pausing more frequently.

And B.:

D. 213. Imitation of sound ('da-da') to-day, on three occasions within one hour, though two or three times it failed. He had been saying 'da-da' very distinctly for two days, but these imitations were faint whispers (very characteristic of the beginnings of deliberate imitation of language).

D. 217. When I said 'da-da' and then 'ba-ba' very emphatically to B., he looked at me with great interest, and repeated, after about five or six seconds, 'da-da': and so four times, each time interval about five to seven seconds; once or twice very faint whispers. This speaking seems quite different from his babbling, it is so deliberate, suggestive of effort. But the same test failed on D. 230.

With A.:

D. 234. Imitated clacking noise made with my tongue last night.

D. 235. Unmistakable imitation of clacking noise. Repeated series of about five noises of E.'s, five times out of six.

With Y., nine and a half months:

D. 290. When driving in car, she imitated the electric horn (which sounds jerkily) by a harsh intermittent cough.

Again with B., at eleven months:

D. 354. Repeated 'bow-wow' after us to-day: but I noticed, as often, that he does not attempt to say it as long as E. goes on repeating it with only short intervals, say of two or three seconds. One has to leave longer (five seconds or more) before he will imitate.

This suggests that on such occasions an important factor in imitation may be interest in the thing (sound or action). It is not a mere reflex. The child vaguely wants the sound, etc., and is satisfied if you give it. This, however, certainly does not cover all cases: for a movement is often imitated while it is still being performed by the person imitated. Cases in which it does not apply occur during this same period, namely imitation of movements such as putting the head on one side. I suggest that the sound of the voice produces a fuller realization of the sound than the sight of *e.g.* a head movement produces of head movement.

This imitation of specific sounds towards the end of the first year provides, of course, the essential basis for the learning of the mother tongue, and will be studied more exactly elsewhere.

VI. IMITATION OF MOVEMENTS: SIX TO TWELVE MONTHS.

Let us turn back to the imitation of movements other than speech. The first suggestion is in A.

D. 182. E. was waving a handkerchief about and A. showed great delight, leaning forward to watch, and then he began to wave his arms about.

B. on D. 186. Very interested in my playing a harmonica. Wanted to grasp it. E. gave him the stick but he dropped it to grasp the metals. Again she gave the stick to him and at once he moved his arms up and down so that the stick struck notes; this time he held the stick for ten or fifteen strokes, and so later, as long as the striking was continued. But later he again dropped the stick to grasp the metals, and certainly showed no tendency to strike them himself later.

If the striking was done in order to produce the sound, it was the first example noted of consciously purposive (reflective) imitation: but even so it did not apparently last long.

D. 229. Failed to get him to imitate hand wave, even by waving his hand in reply to E.

In Y. there was certainly a considerable increase of imitation in the eighth and ninth month. At seven and a half months Y. learned through imitation to do 'pat-a-cake'; at nine months she would put her head down sideways in imitation of another, both these things being done as responses so frequently as to put the fact of imitation beyond question.

Between ten and eleven months she added 'waving hands' and offering kisses, not only by making the sound but by putting her face forward; nodding and shaking head; she also imitated stretching up both arms above the head. In the tenth month she imitated the clapping of a friend's hands by taking hold of that friend's hands and trying to bang them together, either a case (I think the earliest) of imitating the *end* rather than the exact movements, or of simply getting the action reproduced by the actor himself. Also she brushed her own hair with her own brush, the first correct use of a tool, whether intelligent or not.

At eleven and a half months Y., after seeing a brother turning somersaults, with his head on a cushion on the floor, twice lay down, and, after laughing, put her head on the cushion.

When Y. had been fed with apple from a spoon, E. pretended to give some apple to a doll held by Y. Then E. gave the spoon to Y. who promptly put the spoon, though the wrong end of, it to the doll's mouth.

Several new imitations were noted also in B. at this period. The notes, including failures, are as follows:

B. at eleven and a half months:

D. 358. B. on my knee. I raised my head towards ceiling and puffed out smoke, he imitated exactly. E. timed the intervals four times between the beginning of my puffs and his responses. Times: 6 sec., 6 sec., 7 sec., 6 sec., 6 sec.

D. 359. Could not get him to imitate putting up both arms. He simply put up one, the right, even though I several times put up his left arm also (a departure for once, as an experiment, from the rule of not trying to get the child to imitate).

D. 365. When he heard and saw me playing the piano he came and thumped the bass notes. (This, of course, might be purposive.)

D. 371. Did not attempt to copy head shaking.

D. 375. Noticed head shaking to-day, but he would not do it in imitation.

D. 385. Saw me and E. kissing several times. Suddenly put his face forward and put lips to E.'s face and made kissing noises.

D. 387. Does not readily imitate my smoking now or throwing back head: has not done it to-night at all, though I tried to make him, by repeating the puffing in his sight.

But D. 387 seems to have been an exception for B., for I noted on

D. 482. He still imitates my puffing (smoking), so imitation of action goes on apparently as long as action interests.

Of A. I also noted:

D. 391. Copies me putting my head back to 'puff' in smoking, and has done so several days.

This imitation of smoking, puffing and putting back the head, is hard to reconcile with the view that primary imitation is confined to actions prompted by specific instincts[1].

Of course the separate actions of putting the head back and of puffing are sometimes done spontaneously, but the *ensemble* is not an instinctive activity. Nor is it purposive—a seen means of accomplishing a desired end: and this is still more evident in the imitation of reading at fourteen months. Thus Y. frequently in this fifteenth month put her face close to a book and made soft mutterings, as though imitating others reading aloud.

D. 614. B. imitated reading this evening. Sat on floor with book on floor and put his face close to it, moving his head across it as if reading.

This supplements the observation of a considerable number of imitations which can hardly be regarded as 'purposive,' or involving any

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[1] Cf. Washbourne, *Animal Mind*, p. 238.

'insight,' such as crooning, tongue protrusion, mouth gaping, leaning head on one side, hand waving, all occurring before the end of the first year.

To these we must now add puffing and reading aloud. Such spontaneous imitations are clearly distinguishable from others in which, now that the child of one year can understand more of the wishes of others, he can be urged to imitate, as the following note on Y. illustrates.

D. 388. To my amazement I was told that the nurse had taught Y. to wink. I was sceptical. Y. was brought and at E.'s command and example winked definitely with the left eye several times: and so later, in response to my winks, without any command. Sometimes the right eye-lid also moved: but several times I have to-day seen a distinct closing of the left eye without movement of the right eye-lid.

This imitation of winking was frequently noted on the succeeding days; undoubtedly it then became associated with the laughter she thus caused and which she evidently enjoyed, so that within a day or two she winked spontaneously. Such imitations of winking, I imagine, will not be regarded as 'instinctive' and, I hope, not at this age as 'purposive' or 'involving insight.' In this case, however, as the imitatee was the nurse, we cannot be sure that Y., though only a year old, did not understand that she was desired to do what was being done, even on the first occasions. She certainly did when I observed her.

There is no doubt that at this early stage of, say, twelve to fifteen months, one thing will be imitated and another not, without any very obvious explanation. My notes naturally tend to give more frequently cases of imitation: cases of non-imitation were occurring, of course, daily. But none of the notes reveal failures to imitate actions one might have thought likely to be imitated.

A child cannot, of course, imitate an action when the action is so complicated or so remote from his previous actions that it would be almost an entirely new one for him. New combinations and slight modifications can be made, as when 'dad-dad' was said (repeatedly) in imitation, when previously only 'dad' has been uttered. Mere ability, however, does not ensure imitation; for sometimes an action, which is done at times spontaneously, will not be imitated. (See notes D 375 above (p. 113), and also D. 387.)

One would hardly have thought it necessary to call attention to the fact that a child does not always imitate an action which he can do, had not Koffka made the curious statement that the 'compulsion to imitate' actually arises from the ability to imitate[1].

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[1] *Growth of the Mind* (Eng. trans.), p. 317.

So far, then, up to the period of about one year, we can trace a number of individual tendencies of a primary, impulsive type, not implying conscious purposive imitation. Such tendencies to imitate specific actions, no doubt, vary greatly in different individuals[1], but in none do we find a general tendency to imitate anything and everything. Nor are the actions imitated always instinctive in themselves (as, for example, reading, or puffing smoke). They are all *interesting*, otherwise the child would not observe them; but not all interesting actions are imitated.

Some actions, I think, are not imitated because they are already done for a purpose, are definitely associated with that purpose, and only used when that purpose is in mind. Thus of B. I noted:

D. 371. Did not attempt to copy head shaking.
D. 375. Noticed head shaking by B. to-day, but he would not do it in imitation.

Though Y. learned to shake her head intelligently (*i.e.* when she apparently realized she must not have something), I find no note of her imitating the head shake *in vacuo*. And in the series of thirty-eight tests described later, head shaking, nodding, and hand-waving (goodbye) which had acquired definite meaning, were almost the only actions not imitated.

VII. THE LATENT PERIOD AND THE EFFECT OF REPETITION.

Some new aspects appear in imitation shortly after twelve months. Thus the *memory* of a seen action may produce imitation. This was noted in Y. at twelve months, two weeks; when crawling round the room after seeing me open my mouth wide, she several times opened her mouth wide. Of B. I noted:

D. 415. When given a piece of clean paper, he took pencil which he had been playing with and made marks, remembering apparently last time I did it, perhaps two weeks ago.

Memory imitation is also shown in speech, *e.g.* Y.:

D. 386. In middle of slight 'grousing' to-day when she had dropped a tin box she was playing with I heard a distinct 'ta' sound. This had not been said to her, by anyone just before, in fact no one was talking to her. Probably twenty-four hours had passed since she had heard 'ta.' (At a later stage deferred imitations of speech are frequent.)

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[1] *E.g.* Major noted that while his child, at eight months, would shake a paper to make it rattle in imitation of another's action, all efforts to get him to clap his hands, or wave good-bye failed, the last being persistently tried till the baby was twelve months, before the imitation followed. *First Steps in Mental Growth*, p. 128.

Several other examples of the latent period will appear in the record of the experiments given later. Cases occur more frequently at later ages: *e.g.*

B. (2 y. 4 m.) would not imitate J. M. in a somersault yesterday; E. says he seemed frightened at her suggesting he should. But this morning he spontaneously tried it at considerable risk.

The occasional need for repetition of stimulus is also seen in B.

D. 466. E. noted B. imitated my feeding his toy donkey. I held spoon to donkey's mouth and smacked my lips, and he did the same. I have been 'feeding' his toys to make him take his food for over a week and he has only begun to imitate the last day or two.

D. 467. He readily imitated E., raising arms above head and stretching, but more completely after she had repeated it several times, *i.e.* though novelty, no doubt, has its influence, there seems to be some weight due to repetition; the idea is impressed by that—repetition at first seems to increase interest, as indeed it may in the adult, for example, in the sphere of music, and even of pictorial art.

The need for repetition will also be illustrated in some of the tests upon Y. described later (*e.g.* No. 33). It may be regarded sometimes as a necessity if the act is to be apprehended sufficiently for imitation. Or sometimes it may be that the action is made interesting chiefly through repetition and recognition.

VIII. SUMMARY OF EVIDENCE UP TO TWELVE MONTHS AND SUGGESTIONS OF A THEORY OF IMITATION.

It may be convenient at this stage to sum up the evidence we have up to the period of about a year or a year and a quarter, and to sketch a view of the nature of imitation.

Before two months, at an age long before purposive imitation can be imagined possible, we find clear evidence of specific forms of imitation. The earliest is the imitation of sound making, developing later into more and more close imitation of the actual words spoken.

Before three months, smiles and laughter are imitated.

Before six months, the curious phenomenon of imitated tongue protrusion was certainly observed.

Between six and twelve months we find broader movements imitated, requiring muscular co-ordination learned by experience, as, for example, waving hands, putting head on one side, 'smoking', making kissing noise, and winking.

These are all alike in that they are not done as a means to an end of which they are perceived to be the means.

They differ in that: (1) some are actions to which there is a strong pre-existing, innate, specific, possibly instinctive, tendency: as, for example, babbling, smiling and laughing; while (2) some are actions towards which we cannot suppose there is any specific tendency as, for example, 'smoking,' 'winking' and 'reading.'

Some psychologists would not classify the smile response under imitation, but would say that this is an innate expression of feeling, that feeling itself being called forth by a similar expression of feeling.

Others would remove from the list of imitations the speech response—saying that this is the result of the association of the child's own speech utterance with the sound of it; that sound making is a natural, spontaneous impulse, and that the utterance of a sound on hearing it is a case of ideo-motor activity.

But neither of these objections seem to justify the entire separation of the three types of early imitation. It is true that the smile response is peculiar in that it probably has a special feeling attached (though we do not know that the others have not at this stage) and is of such social importance as to justify individual consideration as a specific innate tendency. And the latter is true also of the imitation of sound making. The reference to the ideo-motor theory, however, does not help us much; for, except in a restricted sense[1], it cannot be accepted as universally true[2]; and, further, all forms of early imitation might similarly be labelled. The imitation of sound it may be suggested, is peculiar, in that if it had not been for spontaneous speech causing hearing of sound, the subsequent hearing of sound would not have brought about the speech. Certainly spontaneous speech shows the infant how to do it when the stimulus comes to imitate; but spontaneous movements of hand and head, etc., similarly prepare the way for the imitation of such and more complex movements.

We may accept with McDougall the view that some imitations are due to, or at least closely connected with, the gregarious instinct: though it would in my opinion be better to limit the gregarious instinct definitely to the impulse to get *among* one's fellows, and then further admit a tendency to imitate them in many actions, a tendency also biologically useful.

We may also admit with Thorndike: (i) an original attentiveness of man to the acts and sounds of others; and (ii) the "original satisfyingness of the approval so often got by doing what other men do[3]." Yet we cannot infer from these the absence of imitative impulses. Indeed these principles of Thorndike surely go some way towards supplying the foundations of even a *general* tendency towards imitation, which seems contrary to facts, and which Thorndike himself denies.

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[1] As indicated under type IV of cases of imitation given below.

[2] See McDougal's *Outlines*, p. 290.

[3] *Op. cit.* p. 117.

We cannot, I think, find *one* formal definition of imitation or one formula to cover all cases. The problem is partly one of exact defining of terms, rendered more difficult by the rich complexity of human nature, and especially by the over-determination of many actions by several motives blended, and by the tendency for nature to avoid sudden jumps. My own conclusions at this stage I may sum up here briefly, giving a preliminary grouping of types of imitative response.

It may first be admitted that there seems no reason to believe in a general tendency to imitate anything and everything, seen or heard. An infant is constantly being presented with examples of possible actions which he does not imitate, and to which he does not even attend except momentarily.

As to positive tendencies:

(i) There are specially strong innate tendencies to imitate certain specific things, which are necessary or useful for social intercourse, *e.g.* expressions of feeling, such as smiling and crying; and speech sounds.

(ii) There is a strong tendency to imitate when the action seen appeals to some pre-existing natural impulse, provided the situation of the moment is not unsuitable for it to be called forth. The examples of this chiefly come at a later stage than the first year. The cases overlap somewhat with the preceding type (i): there is, for example, a strong spontaneous tendency to babble and perhaps also to smile; but the former greatly declines in deaf children and the faces of the blind are well known to become less expressionful.

Many imitations, however, cannot come under (i) and (ii) above. There is no 'strong innate tendency' to throw the head back and puff as in smoking or to wink or to read aloud, nor is there a special social value in such imitations. But they, and other actions like them, would be curious actions to a little child, and they are obviously very interesting to him.

Now the imitation in such cases may be one of two types.

(iii) First it may be *the issuing into action of an idea very vividly conceived, which monopolizes attention for the moment*—as the phenomena of hypnotism are usually explained, and as may occur even in adults. For example, a snapshot I took of a high jump at some college sports reveals the Principal of the College standing near the bar, with bended knees, unconsciously imitating the jumper. With the restricted range of attention of infants it is natural to suppose that such monopolization of the whole field of attention will very frequently occur.

(iv) A second type to which such imitations may conform is one in which actual *imitation of the action serves the purpose of making the action more vividly realized.* This type again may be found in adult behaviour. For example, I sometimes find myself, in reading a vivid description of a character in fiction, imitating some mannerism or action of feature described. This theory fits in readily with many of the observed facts[1]. It will be remembered that B. imitated actions of things seen in pictures and, at a later stage, actions described in stories, as they were told to him. This was also observed of Y., as it has been of many children.

In both (iii) and (iv) the imitation will tend to follow, naturally, only when the child can already perform an action resembling fairly well the seen action. But they are neither of them of a type which necessitates the actions imitated being as one writer says (following Kohler) "of a type already thoroughly familiar and completely understood." Indeed it is for the very purpose of understanding more fully something novel, that some actions of the type of (iv) seem to be imitated.

(v) We still have a few *imitations* of actions which almost seem *of a reflex type*. The tendency to cough when one hears others cough is well known. Here is a reflex responding to a perceived occurrence of the reflex in another, not surprising when we reflect that the flow of saliva at the thought of food affords an example of the stimulation of a reflex even by a thought. One or two examples of imitation given above seem to be of this type, for example, the protrusion of the tongue[2]. The latter might be regarded as the imitation of a very interesting action; for the infants certainly seemed fascinated by it. But it occurred at so early an age, that one can hardly imagine the child in any sense knowing that the protruding tongue corresponds to *his* tongue. If, however, they are of a reflex type, the puzzling thing is to account for their existence by any degree of utility. Still, it may well be that nature is constantly throwing out experiments, as it were, in the fixating of movements as reflexes or instincts; that they appear fleetingly and, if not of use, finally disappear in time, not only in the course of the life of the individual, but eventually from the race, so that they may not appear even temporarily in any individuals[1].

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[1] Stout discusses this 'fuller realization' interpretation of imitation and gives a large place to it (*Manual of Psychology*, Book III, ch. 3). I had independently, as I fancied, drawn the conclusion, from observation of my children, that early imitation was frequently of this type. Then, on re-reading after many years Stout's chapter on Imitation, just before concluding this paper, I found, as I have done before, that an idea I imagined had arisen spontaneously in my mind was contained in that comprehensive book by my former teacher and colleague.

[2] McDougall here applies the term 'extremely simple rudimentary instinct' (*Social Psychology*, p. 106). But what of his emotion—criterion of the true instinct?

Leaving out this last fifth group as rare and doubtful, can we further simplify the classification into four groups? I think we can. In all four cases (given under (i), (ii), (iii) and (iv) above) we may infer a strong interest in the action imitated. The action which corresponds to some 'pre-existing natural impulse' is, no doubt, found specially interesting for that very reason, just as one aspect of an instinct is a tendency to be interested in and attend to certain things which provide a stimulus to the conative impulse of the instinct. In this respect imitative impulses resemble instincts, but differ from them in that the former have more general conditions for their stimulation. The type mentioned under (i) above seems to be fairly specific as regards stimuli, but it is only as regards its special strength and invariability, that the reaction is differentiated from imitative reactions to other, more general or varying stimuli. Finally, the type given under (iv)—(imitation to realize an action more vividly) seems to involve a purpose; but it is not suggested that this is a deliberate and fully self-conscious purpose. Even when I detect myself imitating a mannerism of a character in a book I am reading, I realize that it is not done after conscious decision to act for the purpose. It seems to come as a result of a slight fascination by the nature of the mannerisms, and the consequent imitation enables me to realize the situation better without my having willed that it should.

Still more, in the case of children imitating actions described in stories and pictures, it would seem quite wrong to imagine that at, say, two years of age, the child consciously decides to imitate in order to realize a story more vividly. There is first, I suggest, the impulse to imitate an action which fascinates him—monopolizes his attention—and then the added vividness of realization emphasizes this tendency, in much the same way as a process in learning by 'trial and error' is 'stamped in' by success.

All the various types, then, of imitation seem to belong to the one type, that of imitation of an action which monopolizes the attention for the moment, interest being sometimes secured through novelty of the action seen, or a strong liking for the agent, a point which will be demonstrated in some experiments described later. Further, the imitative tendency is supplemented at times under certain specific conditions as, for example, when there is already a strong pre-existing innate tendency to perform the action seen.

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[1] Cf. the evanescence of the arm-raising reflex (when an infant is startled) and of the walking reflex (which appears during the first few weeks in some children). These I have discussed in a paper "Reflexes in early childhood; their development, variability, evanescence, inhibition and relation to instincts," *Brit. J. Med. Psychol.* 1927, VII, Part 1.

It will be seen that we have arrived now at a view of primary imitation. very similar in its central element to one aspect of McDougal's view, expressed in his *Social Psychology*, but to which he seems to attach less importance in his later *Outline of Psychology*.

As regards Thorndike, if we emphasize in all these types of imitation the impulse to a similar kind of activity, rather than the *exact* copying of a series of movements (which latter, as Thorndike points out, is far from being the case), we shall avoid one of the main objections which Thorndike himself brings against the view that a tendency to imitate exists, on the ground that many imitations of actions are learned. He writes: "Of course, after he (the child) has performed many acts as segments of many situations, the latter including often the perception or idea of the act, you may frequently, by performing an act, get him to perform it also. But his act is then a result of learning, not of instinct[1]."

True, 'learning' explains the exactness of the copy, but it does not explain the *impulse* to imitate, unless we approve (and surely Thorndike would not) of Koffka's dictum, already quoted, that the ability to do *x* *prompts* imitation of *x*. It is not indeed necessary for Thorndike to hold this in order to maintain his main positive assertion as to imitation; namely, that there are certain 'probable cases' of "production, by behaviour witnessed, of similar behaviour."

A description of a series of experiments which throw further light on these suggested types and theories of imitation will be given in a following paper.

IX. EXPERIMENTS ON IMITATION AT TWELVE MONTHS.

I will give now an account of a series of tests, made on Y., at twelve and a half to thirteen months, to see if further light could be obtained on the question of a specific tendency to imitate. New actions and familiar ones were performed indiscriminately.

The tests illustrate further some of the points I have touched on. In the second column it is stated whether the particular action had been previously done by Y., either spontaneously or in imitation. If known to have been done, it is important as proving the capacity to do it, and the lack of entire novelty. It is important in such tests of imitation that nothing should be said to the child urging him to do the same, and that he should not have a single word of praise for imitating.

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[1] *Educ. Psych.* I, 110.

Tests of imitation on Y., aged twelve and a half to thirteen months

Nature of action done in front of Y.	Whether previously done by Y., spontaneously or in imitation	Result
Test 1		
Putting two hands above head, held straight up	Occasionally puts hands to top of head	No effect at first; beckoned for her supper. When a little of this taken, suddenly put both hands up (without further demonstration), and so repeated. Suggestions of this latent period appear in other tests also
Test 2		
S.[1] sat on floor in front of Y. and banged floor with foot	Not definitely recorded but almost certainly done spontaneously	Y. watched with apparent interest. After a few shuffles and smiles a foot banged; first left foot and later right, and so several times
Test 3		
Kissing noise made	Learned by imitation and now often done spontaneously	Kissing responses after three of mine
Test 4		
Building up three bricks	Never done before, though Y. must have seen bricks being built up	Seemed interested, but only banged bricks together
Test 5		
E. knelt and put her head on ground three or four times	Never, but has imitated C. doing somersaults	Laughed at E. and watched with interest but no imitation. E. repeated, no imitation
Test 6		
I put my watch to her ear till she heard the tick (and smiled). Then I put watch in her hand	Tested several times during last month or two, without result	When watch put into her hands the second time she put it to her cheek just under same ear (L.). Held it out to A. and made clicking noise. Later put to my face; no effort to get it near my ear
Test 7		
I opened my mouth wide to her	Tested once at least before. Signs of annoyance had then followed, she banging her hand to and fro at my mouth	Hand waved to and fro at my mouth (as when angry) but then her mouth opened wide; so several times
Test 8		
I tore a newspaper several times and then gave it to her	Done months ago, but not recently so far as known	Tried to pull at newspaper, but no tear

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[1] S. here, as elsewhere, stands for 'Sibling'—a brother or sister of the child concerned.

Nature of action done in front of Y.	Whether previously done by Y., spontaneously or in imitation	Result
Test 9 Putting on a hat	Done months ago, but not recently	Tried to put hat on her own head; first time did not manage, did not try again. N.B. Major's child of twelve months tried to put hat on Major (*op. cit.* p. 134)
Test 10 Head shaking	Often done	(i) No result; (ii) No result
N.B. Tests 10 and 11 already mean something definite, and are fully 'realized'		
Test 11 Head nodding	Often done	(i) No result; (ii) No result
Test 12 Arms held by E. at right angles at elbow	New	Imitated (report by E.)
Test 13 Knocking over column of bricks	Last seen done by me, three or four days ago at least	When fourth brick put on and my hands away she approached her finger tip gently (as though pointing) and pushed column over. This not repeated though when I built column again
Test 14 Building bricks (four bricks)	Shown several times before but no imitation. (Last time about a week ago)	At once this time put second brick on first and tried to put third on but it fell. So again just after
Test 15 Putting cork in big ink bottle	New, so far as I know	I took the cork out and then put it in in front of her. She immediately tried to fix it in again but did not succeed; second attempt—offered it to me; third, attempted to put it in, then took cork to mouth. Looked at my face at once when she had done it
Test 16 Gave her three bricks (to see if deferred imitation shown)	See test 14	At once tried to put second brick on first; succeeded after several futile attempts. After succeeding crawled away. When called back and three bricks again put on carpet separately, she put second on first and then tried to add third
Test 17 I was shaking my finger at S. who was holding Y. (no intention of test) telling him something to remember	Never done before, though of course she may have seen such pointing	E. suddenly noted Y. was pointing finger and wagging it somewhat similarly. E. did the same; Y. turned to S. and wagged her finger to him and seemed satisfied only when he joined in

Nature of action done in front of Y.	Whether previously done by Y., spontaneously or in imitation	Result
Test 18 Hand wave	Frequent: it already has a meaning for her	No imitation, but offered hand to shake
Test 19 Head shake (No)	Ditto	Imitated with a smile
Test 20 Head nod (Yes)	Ditto	Imitated with a smile
Test 21 Two hands over head	New, so far as known	No imitation, but very busy with a toy
Test 22 As 21, on E.'s knee, no toy	—	Imitated this time
Test 23 Put waste paper basket over my head	New, so far as known	When I put open end towards her, she took hold of each side and bent her head down, slightly inside basket (too heavy to lift up). So again second time; but then it ceased to interest her and she made off for papers on floor
Test 24 E. rubbing cream into her own hands (not deliberate test)	New, so far as known	Movements clearly imitated
Test 25 I opened my mouth wide, facing Y.	See test 7	Opened her mouth (twice) and put first finger in as though pointing. Later simply gaped at me twice
Test 26 When Y. imitated E.'s putting head on one side, I called her attention to me and did the same	Often imitated before	No imitation of me followed. But again Y. imitated E. Then again no imitation of me. Again imitated E. Then again no imitation of me
Test 27 S. twanged toy violin and threw it down (not deliberate)	Probably played with before	Seized violin and pulled its strings; purposive imitation here?
Test 28 Scratched my head	Done spontaneously, no doubt	Hands up to head and slow scratching movement
Test 29 E. bobbed up and down, doing physical exercises	New, so far as E. concerned	Y. laughed and began to bob up and down similarly. So later when I did similar exercises
Test 30 E. was airing garment in front of the stove (not done as test)	No doubt seen occasionally; never done	Y. went and took another garment, toddled to stove and solemnly held it in front of stove like E.

Nature of action done in front of Y.	Whether previously done by Y., spontaneously or in imitation	Result
Test 31 Said ‘ta’ to her, nothing being offered	Often tried. Never had imitation followed. But last night it had been whispered after giving of toy, and later had been practised by her and twice shouted	Imitated at once, softly whispered, but very clear
Test 32 E. took a hand-mirror and patted it, then gave to Y.	Certainly new	Y. took mirror and at once patted it several times, and then gazed into it. Notable that she patted it first because the right side was towards her
Test 33 (*a*) Still trying to get something she would not imitate, I took a book, put it on the floor and put kettle lid on it	Certainly new	(*a*) Lid lifted and thrown behind. Book played with
(*b*) Test repeated	—	(*b*) Same result as above
(*c*) Test repeated	—	(*c*) Lid now taken and placed steadily on book. (Again effect of repetition?)
(*d*) Test repeated	—	(*d*) Book opened and lid put inside. She had just found slip of paper inside and tried to replace it and attempted to close book
Test 34 E. doing “Peter works with one hammer,” then two and three, working finally two hands, two feet and head	Quite new	Laughter. Then one hand lifted and waved and one leg lifted (as she sat on floor) and held aloft. No movement of head
Test 35 Y. sitting in cot. I rested my chin on top side	New	Stuck out her chin in same attitude and comically like expression but not on rail: and so three times
Test 36 E. had her chin on hand (not done as test)	—	Put her hand to chin in exactly the same way
Test 37 E. doing exercises, arms extended, hands gyrated at wrist (not done as test)	Seen a number of times	Y. repeatedly imitated and now does the action easily

It will be seen that in 31 out of 37 tests there was imitation; and in several of the negative cases there was a good reason for the absence of imitation. Thus in test 21, Y. was busily occupied with a toy; when she was without a toy, the same test, 22, was successful. In tests 11, 12 and 18 the nodding and shaking of the head and the hand moving had already acquired a meaning for the child; and it is likely that she would be disinclined to do these things when she was not 'thinking' their mental correlate at the time[1]. Apart from these four tests, the only failure to imitate was in test 5, and test 4—the building up of three bricks—which may have been beyond her capacity at the time, and to which the banging of the bricks may be considered some approach. I may add that about half-way through this series I asked several other members of the household to suggest things that Y. would be likely *not* to imitate, but within her capacity. Yet we failed to find anything.

Now none of these imitations (with one exception) could be considered 'reflective' (consciously purposive) or done with 'insight' into any value or end. There may, no doubt, be in some a satisfaction in doing a novel thing (*e.g.* test 15). But that is, I suggest, an aspect of one of the types of imitation given above.

It is possible there was more imitation by Y. than by any other of my children, though I cannot be sure that the others were less imitative (especially B. and X.) in the absence of exact tests. Lest Y., however, might be considered quite exceptional in her tendency to imitate, I gave a colleague the list of tests given above and he kindly undertook to apply them to his boy, aged 1 y. 10 m. This colleague, I may add, is a competent psychologist, with training and considerable experience in experimental work and in testing young children. I give below a summary of the results[2].

X. EXPERIMENTS ON IMITATION AT TWO YEARS

All but 4 of my 37 tests were given by Mr C. Out of the 33 done, 25 were imitated—in two cases with a latent interval. Of the 8 in which no imitation followed:

(*a*) Two were the 'head shaking' (meaning 'No'). In the first of them the child "looked puzzled—stopped playing with book and pointed to bricks, no doubt taking the head shaking as meaning prohibition." In the second case also he looked puzzled.

(*b*) One was the head nodding—"the child turned to his toys laughing."

(*c*) One was the hand wave—"which has a meaning—the child said 'No.'"

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[1] Cf. Dr T. Ischikawa's remark that the growth of association paths makes imitation 'harder.' *Beobachtungen uber die geistige Entwicklung eines Kindes.* (Beyer and Sohne, 1910.)

[2] My thanks are due to Mr A. E. Chapman for so carefully carrying out these tests.

(*d*) One was the saying of 'ta' (No. 31) which was not imitated but *obeyed*, the child offering the toy he was playing with.

This leaves only three cases of failure to imitate when the stimulus did not *mean* something, viz. Nos. 28, 32 (in which the mirror was wisely looked into instead of being patted) and 36, which might be regarded as too commonplace an action to be interesting at 1 y. 10 m.

Before the experiments were done on Mr C.'s boy I was anxious to see if the strong tendency to imitate was still present in Y. to the same degree at two years of age. So most of the tests given at one year were repeated on Y. at 2 y. ½ m. in a series extending over three months.

In these I also tested the influence of the person imitated. It will be noted that, in one or two of the first series of tests at one year, Y. only imitated when her *mother* performed the action. In this second test series I sometimes arranged that others should first perform the action and then, if no imitation followed, her mother (E.) repeated the action. The number of each test indicates which test it was in the earlier series.

Experiments of imitation on Y. at 2 y. 0m. to 2 y. 3½ m.

No. of test and person doing it		Result
1 (i)	S.[1]	No imitation
(ii)	F.	No imitation
(iii)	E.	Imitated
2	E.	Imitated
3	E.	Got up and kissed E. When E. persisted, said "Don't do that," then got up and kissed her again
4	S.	Imitated
5	E.	Imitated
6	F.	Imitated
7	F.	Looked at me silently
	S.	Said "Don't"—so several times
	E.	Said "No, mummy"—then her mouth gaped open and so a second and third time
8	E.	Took torn paper but no further tearing
9	E.	Imitated
10	E.	Laughter
11	E. (head nodding)	"Don't do that," repeatedly
12	S.	Imitated
18	S. (hand move)	No imitation
	E.	No imitation
23	S.	Imitated
25	F.	Said "Funny Daddy—Mummy do it," then did it herself
29	E.	Imitated
34	E.	Ditto
36	E.	Ditto

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[1] S. throughout stands for Sibling and is used when the use of the usual letter might indicate the relative ages of the children. In these experiments F. means the writer of the paper.

	New additional tests	Result
40	Arms stretched up, F. Arms stretched up, E.	Looked and said "Mummy do it," then put her own arms up
41	S. opened mouth to show throat to E.	Imitated[1]
42	F. lay on back and did leg exercise	Amused and said "Look Mummy," but no imitation
	E. lay on back and did leg exercise	Imitated
43	E. lay back on floor	Imitated
44	E. was sitting on floor with hands clasped on knees	Imitated, with some difficulty
45	Miss G., living in the house and well known to Y., put two bricks on her own head	No imitation, Y. puts bricks back in box
	E. did the same	Imitated
46	F. puts a brick on his foot	Y. goes on packing up bricks, which she is eagerly enjoying
	E. puts a brick on her foot	Packing continued
47	Miss G. put brick on her own head	Imitated
48	Miss G. held brick under her chin	Y. laughed and put one under E.'s chin then under her own, then one under S.'s chin and again under her own
49	Miss G. put piece of wood in her slipper	Y. took it out and tried to do same

This second series of tests, extending from 2 y. ½ m. to 2 y. 3 m., showed two things.

(i) The strong tendency to primary imitation (of actions with no 'purpose' beyond themselves) remains undiminished at two years as compared with one year. Of the 28 things done, leaving out again the significant hand waving and head nodding, all are imitated except three, Nos. 8, 10 and 46, and in this last test Y. was keenly enjoying packing bricks into a box.

Similarly in everyday life she continued to imitate a number of things which served no other end for her; for example, she often sat imitating 'reading aloud' from a book, during the period 2 y. ½ m. to 3 y., and even after that.

(ii) Y. will sometimes imitate her mother when she will not imitate others, see tests 1, 7, 25, 40, 42 and 45. Compare also test 26 in earlier series.

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[1] This gaping open of the mouth seemed to have a peculiar fascination for Y. She shuddered at it—but imitated, at about 12 months. She imitates again at two years; and as late as three and a half years, when I stood in front of a loud speaker when a speech was being broadcasted, and opened my mouth as though I were the speaker, she watched earnestly and then opened wide her own mouth.

XI. THE IMPORTANCE OF THE IMITATEE.

We see then, that, while Y. will imitate many actions performed by others, it is her mother who 'sets her off,' as none other can. It is, I suggest, the mother whose actions are supremely interesting and with whom especially the child delights to be in accord.

The others whom Y. would sometimes imitate but sometimes refuse to imitate, were also members of the house. I have little doubt that the appearance of a relative stranger would usually so embarrass a child that the stranger would not be imitated to nearly the same extent—that is, at an age when the child is made shy by the presence of a stranger[1]. This ignoring of the importance of the imitatee, weakens the observation of some experimenters, that in the laboratory they have frequently tried to get imitation of simple movements (like putting two hands together) in children of from ten to eighteen months, without much success.

The partial dependence of imitation on the imitatee no doubt also partly accounts for the striking divergence of our test results from Thorndike's statement quoted on the first page of this paper, to the effect that nine times out of ten an infant does not imitate a simple action. My own tests and those I quote from Mr C. show that it is almost exactly nine times out of ten that these children *did* imitate, when they were under normal home conditions.

(iii) A further curious fact, too, was observed in the tests on Y., namely that sometimes she made her mother perform the action first done by another, before she, Y., would imitate it.

At other times she showed a similar desire for others to do the thing that E. and Y. herself were doing, as in test 49 just mentioned. Again, at 2 y. ½ m., when S.'s friend M. was sitting leaning forward with chin on hand, Y. said "S. do it" and was satisfied. The note follows: "She often wants a third person to do likewise when she imitates another."

Here, I think, we are getting beyond the phenomenon of the simplest primary imitation, though we may regard it as a kind of satisfaction in seeing an action imitated by a third person (even on the primary, 'purposeless' level), similar to that gained by satisfying the impulse to imitate another oneself.

When the child herself imitates and gets someone else to imitate, it may well be an example of an action prompted by the original satisfaction of doing what others do, and that this satisfaction is increased when the 'others' are increased in number, "the more we are together," indeed. If one makes a slight, but important modification of Thorndike's phrase quoted above as a main source of imitation (viz. "the original satisfyingness of doing what others do" instead of "the original satisfyingness *of the approval* (italics mine) often got by doing what other men do") we get, I think, very near to the truth of this supplementary aspect of primary imitation, an aspect probably more important in the third year than in the second. Instincts and innate impulses rarely, if ever, work entirely alone except perhaps during the first few months of life. By two years, the tendency for the presentation of immediate and fascinating action to issue in action may very well be supplemented—and almost supplanted, by the growth of an aspect of the gregarious impulse, or of something closely related to it, namely the enjoyment of being at one with others, of being and doing as they are and do, and especially of being thus at one with the beloved mother or father or sister. Yet the fact that Y., *without herself imitating*, sometimes got a third person to imitate the second, suggests that this satisfaction in doing what others do is not the sole supplementary impulse at work on such occasions.

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[1] At a later stage, or when the first shyness is overcome by a period of friendly intercourse, the fact that a stranger whom the child finds attractive is doing something may conceivably make it *more* likely to be imitated.

XII. PRIMARY IMITATION AMONG ADULTS.

Purposeful or 'reflective' imitation begins to appear at least soon after the age of twelve months, when means to a desired end which others are seen to use, may be imitated by the child; and from the age of three years the frequency of primary, purposeless or non-deliberate imitation seems to decrease. Yet it certainly appears even in adult life, as I have already indicated, and it is possible that its frequency and influence is much greater than is commonly thought. Under the heading of 'suggestion' some psychologists have recognized its effect, though chiefly in the domain of thought and belief.

Perhaps the best illustration of primary imitation after infancy is to be found in the involuntary imitation of mannerisms. I noticed that two of my boys, each of whom, at periods separated by about two years, studied under a master who had the habit of sniffing slightly, themselves acquired the habit while in his form, but dropped it shortly after leaving the form.

I invited my class of University graduates to report (anonymously) examples of involuntary imitation which they had themselves indulged in since early childhood; 23 women and 9 men gave instances of their own, out of 38 women and 44 men.

The relative numbers are interesting in view of the fact that the observation of little children suggests that girls are more imitative than boys[1]. Similarly, in a second group of 43 women and 31 men, the recollection of primary imitation was reported by 35 women and 14 men.

The examples given refer chiefly to mannerisms of speech or facial expression and to the imitation of movements seen when watching games, but there are also a number of references to 'unconscious' imitation (as some of them called it) of the handwriting of some admired person. There was further evidence, too, of the familiar tendency of some persons to fall into the peculiar accent of a district to which they have removed.

To trace the possible influence of involuntary or purposeless imitation in adult life would however take us too far from the main object of this paper, which has been to supply evidence of its existence as a genuine innate tendency.

XIII. SUMMARY OF RESULTS AND CONCLUSIONS.

1. The wide divergence of the views of leading psychologists about imitation demands a thorough enquiry into the facts as to the beginnings of imitation during infancy.

2. Actions to which there is already an innate tendency are imitated very readily within the first few months, *e.g.* sound making and smiling.

3. Actions which can serve no purpose perceived by the child and which are not based on instinctive impulses, are imitated freely between the ages of nine and twelve months.

4. There is sometimes a 'latent' period between the seeing of an action and the imitation of it, and sometimes the repetition of an action is necessary before imitation follows.

5. Primary, involuntary, or purposeless imitation seems to be due to the monopolization of attention for a moment by some fascinating impression.

6. Some imitations seem to serve the purpose of helping the subject to realize or enter into the experience of the imitatee more vividly.

7. Some imitations are of a reflex type, if the term reflex can be applied where sight provides the only stimulus.

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[1] This was certainly my impression of my own children, especially in reference to language. Stern asserts quite confidently that girls are more imitative than boys.

8. Experimental tests at twelve months and at two years suggest that the tendency to imitate any very interesting action is very strong in some children.

9. The imitatee is important: *e.g.* a child will sometimes imitate its mother but not another person.

10. There is some evidence that a child may wish others to join in the imitation.

11. The testimonies of two large groups of students show that primary, involuntary imitations are recognized as having taken place in adult life in a substantial proportion of cases, especially among women.

12. The evidence as a whole seems conclusive against the view of some *Gestalt* psychologists that imitation is always purposive.

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CHANGE AND DECAY IN THE VISUAL MEMORY IMAGE

BY GORDON W. ALLPORT.

(*Dartmouth College.*)

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I. PROBLEM AND METHOD.

THIS study is concerned with an examination of the characteristic lines of change and decay in the visual memory images of children. Its theoretical interest centres chiefly in the bearing of its findings upon the law of *Prägnanz*, according to the Gestalt school, the most general law of memory[1]. The investigation is similar in purpose and method to research previously published by F. Wulf[2] and J. Gibson[3]. The methods employed in these three studies may profitably be compared.

Subjects: Wulf, 6 adults; Gibson, 6 adults; Allport, 350 children, average age 11 y. 4 m. (range, 10–13 y.).

Type of stimuli: Wulf, simple, unfamiliar designs of 2 to 13 lines; Gibson, simple, unfamiliar designs of 3 to 8 lines; Allport, Binet (*X*, 3) designs (truncated pyramid with 12 lines and Greek key with 13 lines). Although no more familiar than the figures used by the other investigators, these designs were more closed and more geometrical in appearance.

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[1] Cf. R. M. Ogden, *Psychology and Education*, 1926, pp. 235–238; also W. Kohler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920, p. 259.

[2] Beiträge zur Psychologie der Gestalt (edit. by K. Koffka); VI. "Ueber die Veränderung von Vorstellungen (Gedachtnis und Gestalt)," *Psychol. Forsch.* 1922, I, 333–373.

[3] "The reproduction of visually perceived forms," *J. of Exp. Psychol.* 1929, XII, 1–39.

Number of designs: Wulf, 26; Gibson, 28; Allport, 2.

Size of designs: Wulf, *cir*. 1 in. × 2 in.; Gibson, "maximal dimension for any figure was ⅞ in.'"; Allport, total size for two designs, 7 in. × 2½ in.

Method of exposure: Wulf, cards placed before subjects; Gibson, Ranschburg memory apparatus; Allport, cards on pupils' desks.

Exposure time: Wulf, 5–10 seconds; Gibson, 1½ seconds for each design in a series of 7 or 14; Allport, 10 seconds for two designs on one card.

Number of reproductions secured: Wulf, 400; Gibson, 4000; Allport, 1000.

Time of reproductions: Wulf, after 30 seconds, after 24 hours, after 1 week, and in some cases after 2 to 8 weeks; Gibson, immediately after a series of 7 or 14 designs had been exposed; after 5 weeks, after 1 year; Allport, immediately after exposure, after 2 weeks, after 4 months.

Other conditions: Wulf and Gibson worked with subjects individually; Allport used school-room classes. The subjects used by Wulf and Gibson knew they were to be called on to reproduce the designs; the school children had no warning. Introspection was secured by Wulf and Gibson, not by Allport.

Each of these three studies aims to discover the characteristic alterations in the form of a visually perceived figure after the perception has passed into a trace or engram in memory, and has remained there with little re-arousal and no correction for some period of time. Each investigator tries to detect the change that occurs in this memory content by securing from the subject, after various intervals of time, drawings of the original stimulus as he remembers it. It is not strictly correct to regard the discrepancy between the drawing and the original *Vorlage* as indicating alteration exclusively in the visual memory image. Neither Wulf nor Gibson make such a claim, since the reproduction of the design may be in part a product of motor imagery, verbal conceptualization, or even some more ephemeral *Wissen*[1]. The presumption in the present investigation, however, is that the results bear *primarily* upon visual retention, since the designs were such as to exclude 'object assimilation' (that is, retention through their resemblance to familiar objects), and probably were too complex for verbal conceptualization by a child. The instructions for the reproductions asked the children to "see again in their mind's eye" the original card and to draw what they 'see.' Furthermore, the entire research took place in connection with other experiments on eidetic imagery in which the emphasis throughout was upon visual retention[1].

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[1] Wulf, *op. cit.* pp. 343 f.

The material for the present study was gathered in 1923–4 from elementary school children in Cambridge, England. The experimenter placed face downwards upon each pupil's desk a white card, 8 in. × 6 in. upon which the two designs were printed in bold black lines. The truncated pyramid appeared on the left when the card was turned over[2]. After looking at the designs for 10 seconds, the time allowed for the test by Binet, the children at a given signal again turned the cards face downwards, and immediately drew the designs from memory upon a sheet of paper, same size as the card (8 in. × 6 in.). They were not permitted to see the designs again. Without warning, the experimenter returned in 2 weeks and again in 4 months, and after providing the same sized paper (in some cases the paper was longer, 8 in. × 11 in.), requested the children to draw again what they had seen on the card.

Three reproductions were thus obtained for 275 cases. The reproductions will hereafter be designated as follows: 1—immediate reproduction, 2—reproduction after 2 weeks, 3—reproduction after 4 months. In the remaining cases (approximately 75) only two reproductions were secured. These cases are not reported in the present study, since they exhibit no new characteristics, and since comparisons are more significant for the cases in which the set of three papers is complete.

II. RESULTS[3].

A preliminary inspection of the papers revealed that 42 directions of change and decay could be readily identified. The changes in each paper were accordingly tabulated under each of these headings, the record generally indicating merely the presence or absence of the type of change in question. In order that the three papers for each child might be listed together, each of the headings had three sub-columns for reproductions 1, 2 and 3, and the names of the children appeared at the left of the tabular sheet. The following headings were employed:

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[1] It was originally thought that the excellence of the reproduction of the designs might facilitate the selection of children with eidetic powers (cf. L. T. Carmichael, "Eidetic imagery and the Binet test," *J. Educ. Psychol.* 1925, XVI, 251–252). The method proved, however, not to be adequate for the selection of *Eidetiker*, for only 3 per cent. of the cases reproduced the pyramid perfectly in all three trials, and only 5 per cent. reproduced the key perfectly; while not more than 1 per cent. drew both designs correctly in all three trials. The percentage of *Eidetiker* among children of this age is presumably far greater (cf. H. Zeman, *Zsch. f. Psychol.* 1925, XCVI, 226, and G. W. Allport, this *Journal*, 1924, XV, 101, 104). Even if we confine our census to the 'T-type,' the correspondence is not close enough to be even suggestive.

[2] Burt following Binet's instructions uses the designs in this arrangement, while Terman places the Greek key, the more difficult, at the left; C. Burt, *Mental and Scholastic Tests*, 1921, p. 56, and L. S. Terman, *The Measurement of Intelligence*, 1916, p. 261. The designs in the present experiment are somewhat larger than those used by Terman or Burt. Terman's two figures together measure 5½ in. × 1½ in.; the dimensions of the figures used by Burt are 5 in. × 1½ in.

[3] The writer is indebted to R. G. Morrill and E. N. Scadron for assistance in the tabulation of these results.

A. General changes. Excellence of reproduction: pyramid perfect, key perfect, test passed. Size alterations: pyramid increased, pyramid decreased; key increased, key decreased. Displacements: pyramid and key interchanged, key upside down or on side. Assimilation and omissions: pyramid affected by key, key affected by pyramid, both in one drawing; pyramid omitted, key omitted.

B. Analysis of pyramid. Broad strip displaced, strips equalized, rectangle changed to square, to cube, oblique lines omitted, central rectangle omitted, telescoping, change to oblique parallelogram, presence of curved lines, simplification (less than 12 lines), elaboration (more than 12 lines), disintegration (lines fail to close), and remarks.

C. Analysis of key. Simplification, elaboration, under-emphasis of loops, exaggeration of loops, only loop quality left, exaggeration of zigzag quality, only the zigzag left, under-emphasis of stalk, over-emphasis of stalk, error in direction of one loop, errors in two loops, telescoping of form, disintegration, presence of curved lines, remarks.

The results will be briefly reviewed under these headings, excepting where the data from certain of the divisions can be more advantageously considered together.

A. *General changes.*

(1) *Excellence of reproductions.* In the first trial 31 per cent. of the pyramids were accurately reproduced, 12 per cent. in the second trial, and 6 per cent. in the third trial; in the case of the key the figures for the three trials respectively are 26, 12 and 8 per cent. In all three reproductions the pyramid was correctly drawn in only 3 per cent. of the cases, the key in 5 per cent. Scored by the Terman standards, 63 per cent. passed the test at the first reproduction, 42 per cent. at the second, and 26 per cent. at the third.

(2) *Alterations in size.* Table I shows a marked tendency for the first reproduction to be smaller than the original, and for still more cases to show shrinkage with the passage of time.

Table I. *Size of reproductions (compared with original designs).*

Reproduction	Pyramid: Equal or larger (%)	Pyramid: Smaller (%)	Key: Equal or larger (%)	Key: Smaller (%)
1	22	78	34	66
2	8	92	27	73
3	5	95	22	78

A further tendency not shown in the table was the *progressive* shrinkage of the image for the individual child. When the successive reproductions were compared, not with the original designs but with each other, evidence was found of *continued* shrinkage of the pyramid in 49 per cent., and of the key in 46 per cent. of the cases. No alterations in size were recorded unless they were marked by at least a 20 per cent. increase or decrease.

(3) *Displacements.* In the first trial 4 per cent. of the reproductions showed an interchange of position for the pyramid and key; in the second trial 14 per cent. were interchanged, and in the third 21 per cent. As for inversion of the single figure, it was not possible to tell whether the rectangular pyramid was inverted, but inversion or tilting on one side occurred in the key in 9 per cent. of the first reproductions, 15 per cent. of the second, and 19 per cent. of the third. *Raumverlagerung* is a familiar phenomenon in children's drawing[1]; from the present investigation it appears that it has a tendency to increase with the lapse of time.

(4) *Assimilation and omission.* In a few cases the features of one of the designs appeared to be incorporated into the drawing of the other. The pyramid was noticeably affected by the key in 0·4, 1·5 and 3 per cent. of the cases; while the key showed the impress of the pyramid in 10, 13 and 15 per cent. of the reproductions (see Chart, 2). Just why the key should absorb some of the characteristics of the pyramid more often than the pyramid absorbs the key is not clear. It may be due to the nature of the figures, or to the fact that the figure on the left has a favoured and more stable position. Gibson finds figure assimilation the most frequent kind of change in his experiments, occurring in 16 per cent. of the cases, but he believes his method was "especially favourable for the influencing of one figure by another[2]."

On no occasion were both designs assimilated into a single drawing.

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[1] Cf. W. Stern, *Zsch. f. ang. Psychol.* 1909, II, 498–526, and P. Meyer, *Zsch. f. Psychol.* 1913, LXIV, 46.

[2] Gibson, *op. cit.* p. 25.

A sense of *two* designs was almost universally retained. In no trial did more than 1 per cent. of the subjects omit either figure, and in these few cases the remaining figure did not show any evidence of ***Mischwirkung.*** Both Meyer[1] and Gibson[2] found occasional fusion of two or more figures into one, but their serial method of presentation is probably responsible.

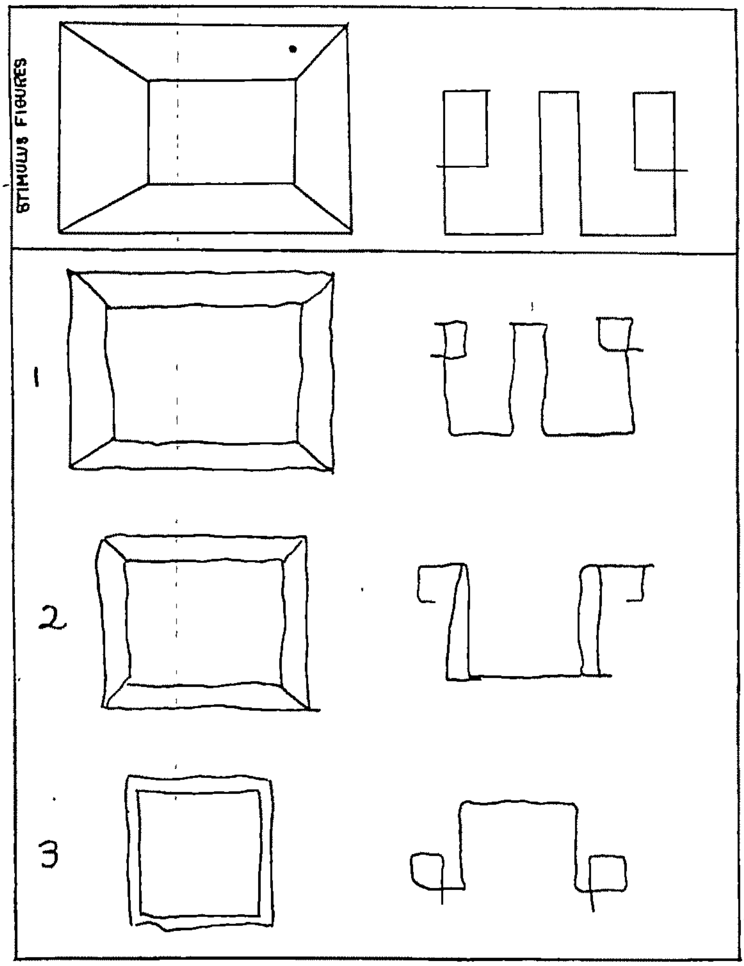

Chart I. The stimulus figures and a typical series of reproductions[3].

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[1] Meyer, *op. cit.* p. 42.

[2] Gibson, *op. cit.* pp. 23, 26.

[3] The designs and the reproductions are approximately one-third their original size. The rows numbered 1, 2, 3 contain the results of the reproductions immediately after exposure, after 2 weeks, and after 4 months, respectively.

The changes described in the text that are illustrated in this series of reproductions are the following:

General changes: failure to pass test after 1; progressive shrinkage in pyramid; displacement of key (upside down) in 3; key affected by pyramid in 2.

Analysis of pyramid: equalizing of strips in 1, 2, 3; changing rectangle to square (progressive); omission of oblique lines in 3; progressive narrowing of strips; simplification in 3.

Analysis of key: simplification in 3; under-emphasis on loops in 2; under-emphasis on stalk in 2, 3; errors in direction of two loops in 2, 3.

Remarks: greater simplicity and symmetry characterize the successive reproductions.

B. *Analysis of pyramid.*

(1) *Displacement of broad strip.* In the first reproduction 15 per cent. of the subjects placed the broad strip on one of the three sides of the figure where it did not belong; in 2, 19 per cent. of the subjects; and in 3, 30 per cent. of the subjects.

(2) *Equalizing of strips.* There is a marked tendency for the subjects to lose their impression of asymmetry of the figure, and for all four strips to become equal: in 1, 39 per cent.; in 2, 52 per cent.; and in 3, 56 per cent. If we add these cases of equalized strips to the cases reported above where strips were displaced, we have totals for what might be called 'strip trouble' of 54, 71 and 76 per cent. These large percentages indicate a conspicuous instability in an asymmetrical figure, a phenomenon which Kuhlmann and other investigators have previously reported[1].

(3) *Changing rectangle to square or cube.* The percentage of cases in which the outside dimensions of the pyramid become equalized, so as to produce a square design are for the three reproductions respectively, 10, 13 and 40. It is perhaps noteworthy that the longer period of oblivescence was accompanied by the chief increase in symmetry. Of the 27 per cent. of cases producing a square in the third reproduction, but not in the first or second, the majority showed a slight but unmistakable tendency toward this form of alteration in the earlier reproductions. In very few instances was there tri-dimensionality in the reproductions, a cube-quality appearing in only 0·3, 0·7 and 1 per cent. of the cases.

(4) *Omission of oblique lines.* This change occurred in only 0·3, 4 and 8 per cent. of the cases.

(5) *Omission of central rectangle.* This change rarely occurred. Two rectangles, one enclosed within the other, remained in all excepting 0·3, 2 and 3 per cent. of the cases. Thus the inner rectangle appears to be somewhat more stable in the image than do the connecting oblique

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[1] F. Kuhlmann, *Psychol. Rev.* 1906, XIII, 347; Wulf, *op. cit.* pp. 352, 356; Meyer, *op. cit.* p. 42.

lines. There was little tendency for telescoping of form to take place, *i.e.* the carrying of the oblique lines all the way across the figure, the percentages being 0, 1 and 2. There was a striking tendency, which is difficult, however, to state in quantitative terms, for the remaining inner rectangle either to shrink, thus magnifying the strips, or to expand (see Chart I), with the result that the strips become very narrow.

(6) *Change to oblique parallelogram.* Such a change would enhance the asymmetry of the figure by causing right angles to become oblique. This phenomenon practically never occurred, the percentages being 0, 0·7 and 0·7.

(7) *Presence of curved lines.* Not only are right angles retained but also the rectilinearity of the figure. Curved lines appear in only 0·3, 1 and 0·3 per cent. of the cases[1].

(8) *Elaboration or simplification.* There is virtually no tendency for the figure to become more complex in respect to the number of its component lines, the percentages for the three trials being 0, 2 and 3. There is likewise little tendency for the reproductions to contain less than 12 lines, the percentages being 0·3, 8 and 8. In general, the notion that one rectangle was enclosed within another and connected with it by oblique lines was well retained.

(9) *Disintegration.* Another rare type of change, occurring in only 0·7, 1 and 3 per cent. of the cases. The figure was apparently too firmly knit to fall apart in the image.

C. *Analysis of key.*

(1) *Simplification and elaboration.* In the first trial 21 per cent. of the cases showed simplification, in the second trial 32 per cent. and in the third 41 per cent. These results tell nothing about the quality of the change, but indicate a decided tendency toward a reduction in the number of component parts of the design, roughly proportional to the lapse of time. In the first reproduction 13 per cent. showed elaboration, in the second 15 per cent. and in the third 9 per cent. Not only is elaboration less common than simplification, but it tends to decrease after a time, whereas simplification increases.

(2) *Under-emphasis of loops.* These percentages represent cases where there was a failure to close the loops (as in Chart I, 2), or where one or both loops were omitted. The percentages for the three reproductions are 18, 31 and 38.

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[1] Gibson (*op. cit.* p. 31) found no cases whatever in which the lines of an exclusively straight-line figure became curved.

(3) *Exaggeration of loops.* This item includes cases where the number of loops was increased or their prominence accentuated (not merely by the omission of the central stalk). This type of change occurred in only 5, 7 and 5 per cent. of the trials. In a few cases, 2 per cent. in each reproduction, nothing was left in the image excepting the loop quality of the design. The records thus show a much more pronounced tendency for an under-emphasis than an over-emphasis of loops.

(4) *Exaggeration of the zigzag quality.* The four long vertical lines in the figure give an effect which might be described as back-and-forth or zigzag. There was some type of over-emphasis (not produced merely by the omission of loops) in this characteristic in 10, 13 and 12 per cent. of the cases. In 6, 9 and 15 per cent. of the reproductions loops were omitted entirely, so that nothing excepting the zigzag quality remained. It would therefore appear that, for purposes of retention, the zigzag quality is the more essential feature of the key.

(5) *Under-emphasis on stalk.* In this category are included both reductions in the size of the stalk compared with the rest of the figure, and omission of the stalk. The percentages are 31, 46 and 57, showing this type of levelling to be a common trend in the deterioration of the image.

(6) *Over-emphasis on stalk.* Duplication, pronounced elongation or other elaboration of the stalk occurred in 6, 5 and 4 per cent. of the cases. This type of sharpening is not common and seems to decrease slightly with time.

(7) *Errors in the direction of one loop.* The percentages are 15, 17 and 10. The decline of this type of error in the third trial is bound up with the next phenomenon.

(8) *Errors in the direction of two loops.* Both loops show errors of direction in 18, 37 and 44 per cent. of the cases. In virtually every instance this change means a simple reversal of both loops, so that instead of facing inward they face outward from the figure. Some children in the first and second trials turned one loop and in the third trial also turned the other.

(9) *Errors in loops in general.* If percentages are combined for under-emphasis, omission, errors in the direction of one or both loops, we find that 'loop trouble' occurs in 54, 68 and 71 per cent. of the cases. This instability of loops is nearly as marked as is 'strip trouble' in the case of the pyramid (B (2)).

(10) *Telescoping of form.* By telescoping is meant some definite intermingling of loops and stalk. The percentages are 7, 9 and 8.

Virtually no cases of analogous telescoping in the pyramid are found (B, (5)).

(11) *Disintegration.* The percentages are 3, 4 and 3. In neither the case of the pyramid (B (9)) nor of the key is decay marked to any significant extent by a 'falling apart' of the figure. There is reason to believe that in the few instances recorded the failure of continuity in the reproduction is due not to incohesion in the image but to accidents of drawing.

Table II. *Summary classification of changes.*

(Expressed in percentages of the total number of cases).

	Reproduction		
Type of change	1	2	3
Assimilation and omission			
Pyramid affected by key	0·4	1·5	3
Key affected by pyramid	10	13	15
Both in one drawing	0	0	0
Key omitted	0	1	0·3
Pyramid omitted	0	0·7	1
Reduction in size			
Pyramid	78	92	95
Key	66	73	78
Displacements			
Interchange	4	14	21
Key inverted or tilted	9	15	19
Chief points of instability			
'Strip trouble' in pyramid	54	71	76
'Loop trouble' in key	54	68	71
Tendency toward symmetry			
Equalizing strips (pyramid)	39	52	56
Becoming square (pyramid)	10	13	40
Both loops reversed (key)	18	37	44
Both loops reversed or unchanged	64	69	73
Sharpening of features			
Exaggeration of zigzag	10	13	12
Only zigzag left	6	9	15
Over-emphasis on stalk	6	5	4
Over-emphasis on loops	5	7	5
Only loop-quality left	2	2	2
Levelling of features			
Under-emphasis on stalk	31	46	57
Under-emphasis on loops	18	31	38
Omission of oblique lines	0·3	4	8
Omission of central rectangle	0·3	2	3
Telescoping in key	7	9	8
Telescoping in pyramid	0	1	2
Disintegration (lines not closing)			
Pyramid	0·7	1	0·3
Key	3	4	3
Presence of curved lines			
Pyramid	0·3	1	0·3
Key	3	3	4
Simplification (in number of lines)			
Pyramid	0·3	8	8
Key	21	32	41
Elaboration (in number of lines)			
Pyramid	0	2	3
Key	13	12	9

(12) *Presence of curved lines.* This phenomenon occurred in only 3, 3 and 4 per cent. of the cases. It was even more rare in the reproduction of the pyramid (B (7)). Here again the hazards of drawing probably exaggerate the extent of this type of change.

Although the two stimulus figures are of very different shape it is not difficult to detect certain types of change common to both, and to summarize these (Table II) under headings that have significance for the theory of the alteration of imagery with the passage of time.

III. INTERPRETATIONS.

To a certain extent the classification in Table II is arbitrary. Gibson, for example, has pointed out that the same feature may be called sharpening or levelling depending upon the point of reference[1]. But even with the caution which this limitation necessitates, it is not difficult to recognize the following unmistakable tendencies in the reproductions, and with these any adequate theory of retention must deal.

(1) *The progressive nature of change and decay.* Köhler writes, "Traces are not rigid; there are definite dynamical tendencies in them[2]." The study reveals two senses in which this statement seems to hold true. In the first place, under virtually all of the headings there is a tendency for more and more cases to manifest the type of change in question as time elapses. This phenomenon seems to mean that there may commence at any time within the dormant memory material typical processes of change and decay. This deterioration sets in at various times during the retention without any apparent interference from without; it seems indeed to possess 'dynamical' and 'self-distributing' properties. In the second place, wherever studied in the present research, the tendency usually exists for a change to persist in a given subject's memory in the same direction in which it starts (cf. A (2), B (3)). It is, however, true that occasionally a trace takes a new form, and appears in quite a different shape in successive reproductions. But in either of these cases it is clear that the change must be conditioned by processes within the memory-activity itself.

(2) *Retention of two designs.* In no case did the two designs merge into one, and in very few cases was either omitted, and in no case were three or more drawn. The most elementary fact in the reproductions seems therefore to be the retention of *two* visual impressions, no matter how sadly their form may deteriorate.

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[1] Gibson, *op. cit.* p. 33.

[2] W. Kohler, *Gestalt Psychology*, 1929, p. 309.

(3) *Reduction in size.* More cases showed a shrinkage in the size of the image (as judged by the reproduction) than in any other single type of change. As many as 95 per cent. of the children displayed at least a 20 per cent. reduction in the size of their third drawing of the pyramid as compared with the original stimulus[1]. Among the associational factors that might be alleged to be operative are (1) the confining effect of the paper upon which the designs were reproduced (but this paper was either the same size as the presentation card or somewhat longer), (2) the habit of writing and perhaps of drawing in small dimensions, (3) a cautious or thoughtful attitude which produced constricted movement. None of these explanations, however, seems adequate to explain the fact that there was so frequently a *progressive* shrinkage in the size of the image from one reproduction to the next, or that more and more cases showed the shrinkage as time elapsed. The phenomenon seems to be more readily explained by the hypothesis that shrinkage with time is one of the definitely 'dynamical' properties of traces to which Köhler refers. Using much smaller figures, neither Wulf nor Gibson report such shrinkage. Hence this dynamical property seems to be confined to a certain (presumably ascertainable) rànge in the size of the stimulus[2].

(4) *Displacement.* Since a fifth of all the children in the third reproduction placed the key on the left instead of on the right, and a fifth inverted or tilted the key, it would seem that another dynamical property of traces is here disclosed. (Technical difficulties, such as fitting the designs to the page, may have accounted for some displacements, but scarcely for so many.) It is interesting to observe that the exact number of designs was virtually always retained, that the position of the designs in relation to each other remained correct in approximately four-fifths of the cases, and that the perfect form of the designs was retained in

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[1] H. K. Wolfe (*Amer. Journ. Psychol.* 1898, IX, 137–166) found that among 415 fourth-grade children, only 1 per cent. drew an American dollar bill from memory as long as the original, and only 4 per cent. as wide. In respect to age of his subjects and size of the stimulus object, his conditions are like those of the present study and his results similar.

He states, however, with considerable assurance a typical associational explanation for the shrinkage of the image: "The explanation of so large constant errors will occur to every one. We seldom see bills spread out at full size. They are generally folded..." (p. 156). The conditions of the present investigation rule out completely this naïve account of shrinkage.

[2] Using figures 10 mm. high, Meyer (*op. cit.* pp. 62 f.) found 16 per cent. of the children's reproductions became smaller and 35 per cent. larger; using figures 40 mm. high, 84 per cent. were smaller and none larger. Meyer considers this a striking result, since adults do not show so marked a tendency to decrease the size of figures, either large or small. The predilection of children for diminutive drawing, she finds, is less dependent than that of adults upon the size of the original stimulus.

only about 1 per cent. of the cases. These results seem to indicate that different aspects (number, position and form) of imagery possess different retentive value.

(5) *Unstable elements in the designs.* In the case of the pyramid it is clear that the greatest instability lies in the feature which is unsymmetrical, that is, in the lateral displacement of the inner rectangle. This situation does not have an exact counterpart in the key whose original form is perfectly symmetrical. In this design it is the loops which are ~~the~~ commonest source of error.

(6) *Tendency toward symmetry.* Perhaps the most striking of all the results is the tendency for the figure to retain, or to achieve, symmetry. Strips in the pyramid were equalized, and the rectangle became a square, with marked frequency, and very seldom was there a loss of the rectilinearity of the figure through the introduction of curved lines. In general, simplicity as well as symmetry was the rule, as shown in the relative absence of acute angles and elaboration; although the notion of an enclosed rectangle was tenaciously retained. In respect to the key, a marked tendency existed for the loops to finish the figure symmetrically, either by retaining their original direction or by achieving some new and equally symmetrical position. It is remarkable that, in the third reproduction, actually more cases had their loops in some symmetrical arrangement than in either the first or second reproduction. There can be no escape from the conclusion that one of the properties of the trace is for it to retain or to acquire symmetry[1].

(7) *Sharpening of features.* It is possible to consider a number of changes in the key as instances of 'sharpening,' or more exactly, in Wulf's term, 'pointing.' Among these the emphasizing of the zigzag quality of the design is conspicuous. The nature of the material probably prevented 'pointing' to a greater extent; broken lines, acute angles and marked asymmetry seem to be the conditions most favourable to this form of deterioration.

(8) *Levelling of features.* The commonest tendency in levelling is under-emphasis on the central stalk and in somewhat fewer cases on the loops. Under-emphasis on stalk is quite opposed to exaggeration of the zigzag quality; a single reproduction could hardly show both tendencies. Thus it seems, as Wulf points out, that some subjects 'point' and some 'level' the same feature, depending presumably upon the nature of their *Auffassung*[2]. In general it may be said that, in the course of deteriora-

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[1] Cf. p. 139, n. 1.

[2] Wulf, *op. cit.* p. 347.

tion, levelling is more frequently evidenced than sharpening, and this phenomenon accounts for the impression of growing simplicity in the successive reproductions.

(9) *Disintegration.* It can be stated with assurance that the decay of imagery is not normally marked by a falling apart of the figure. The change is characterized by an alteration in pattern and not by incohesion in the figure. Wulf and Gibson, too, have shown that unless a break appears in the original figure, no break will appear in subsequent reproductions of it; if a break does appear in the original, the gap will be either augmented or decreased in subsequent reproductions. Continuity of line in an unbroken figure, then, is another fact with which an adequate theory of retention must deal.

IV. THE GESTALT THEORY OF MEMORY.

Wulf found that without difficulty he could classify the great majority of changes under the headings of 'sharpening' (*Präzisierung*) or 'levelling' (*Nivellierung*). These two directions of change are accomplished, he thinks, through one or more of three agencies: *Normalizierung* (resemblance to familiar objects), *Pointierung* (exaggeration of a particular feature), *struktive Veränderung* (properties inherent within the Gestalt). One and all of these agencies of change Wulf considers to be factors in the achievement of *Prägnanz*, the tendency for every configuration in memory to become as 'good' as possible. Gibson quite rightly objects that 'normalizing' at least is clearly an associational phenomenon, and that *Prägnanz* is a totally ambiguous concept[1].

Gibson, finding so much 'object assimilation' (same as Wulf's 'normalizing') and also such clear evidence that the manner of the original apprehension of the stimulus frequently determines the type of retention, is inclined to believe that "the types of change here observed may all be explained by the supposition that the experience of the individual has brought into existence certain habitual modes of perception, and that these perceptual habits, rather than the laws of configurations, condition the changes observed[1]." Thus Wulf and Gibson, although working with similar material under similar conditions, arrive at virtually opposite interpretations of their results, the former favouring the Gestalt hypothesis, the latter the associational. The Gestalt school asserts that, even where associational assimilation is clearly found, the phenomenon is subordinated to the more inclusive laws of configuration, and the associationists make the reciprocal counterclaim. Thus both the new school and the old are at present trying hard to swallow each other.

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[1] Gibson, *op. cit.* p. 35.

On the grounds of parsimony (or at least tradition) it should perhaps, wherever possible, be conceded that perceptual set, associative context, various influences of habit, etc., modify and change the nature of the image and the reproductions. But in the present investigation there seem to be several findings that offer grave difficulty to any associational explanation: the persistence of change in the direction in which it starts, the spontaneous commencement of a change after a lapse of time, the equalizing of strips and the changing of the rectangle to a square, the preservation of the old, or the achievement of a new, symmetry in the position of the loops, the retention of an impression of two, the frequency of displacements, the continuous shrinkage of the image, the rigidity of the rectilinear form, the continuity or cohesion in each of the figures, the relative lack of elaboration, and the tendency toward simplification. Still another feature, too subjective to sustain much weight, and yet important, is the fact, not previously mentioned, that the recognizability of a design often remained even though every portion of it was incorrectly reproduced.

The associationist might, it is true, dispose of some of these tendencies by invoking the aid of his familiar categories. He might, *e.g.* say that the urgency with which the image acquires symmetry is somehow related to associative predispositions that arise from the bi-lateral symmetry of the body. The exact operation of such an associational process is, however, quite as dim as is the hypothetical *Gestaltdrang* toward symmetry. Nor does it seem that attention or perceptual set aid materially in explaining the various types of change listed above, since the question immediately arises as to why symmetry, continuity, simplicity, rectilinearity, etc., are factors of advantage in the original perception. Then too there is the troublesome evidence that changes frequently occur *after* the first or second reproduction, thus indicating activity and change within the memory (and not in the perceptual) process itself.

Unquestionably the stimulus material itself largely conditions the changes that occur in the image. For example, the designs in the present study were accompanied by marked shrinkage in the successive reproductions, whereas neither Wulf nor Gibson, using much smaller stimuli, report such a change. The work of Meyer (cf. p. 144, n. 2) has already shown that this phenomenon of shrinkage stands in some ascertainable relation to the dimensions of the stimulus figures. And again, 'object assimilation' is a frequent type of change arising from the somewhat ambiguous and amorphous figures used by Gibson and Wulf, whereas the firmly knit figures employed in the present study allowed no such associational fusion.

Although in the light of our findings, Köhler's principle of the dynamical properties of traces suggests itself as the most fruitful hypothesis in dealing with change and decay in the memory image, it is desirable to know precisely what these properties are and under precisely what conditions they become operative. Certain changes have been encountered by a number of investigators under a wide variety of conditions, *e.g.* tendencies toward symmetry, toward exaggeration or levelling, toward greater cohesion and rectilinearity; and still other alterations have been discovered in the present investigation. But until these changes have been checked and thus finally established, and the conditions for their occurrence exactly defined, the so-called 'law' of *Prägnanz* will remain vague and unsatisfactory.

V. SUMMARY.

Changes in the images of 275 children, as revealed in their reproductions of the Binet (X, 3) designs after intervals of 2 weeks and of 4 months, as well as immediately after the exposure, indicate that certain typical alterations occur during the period of retention. The images tend, in general, to become smaller, more symmetrical and simpler. The number of designs is correctly retained, also their essential shapes; there are many variations in respect to detail, some of which can be classified as 'sharpening' or 'levelling' of special features. In general, it may be said that although very few are perfectly retained, the figures do not disintegrate nor lose their identifiability.

It seems to be a reasonable hypothesis that there are dynamical processes in the brain which during retention force memory traces into typical lines of change, but to state that these processes lead inevitably to 'goodness' in the image is merely to obscure the issue. Only by listing the specific tendencies and the conditions for their occurrence, as in the present report, can the concept of *Prägnanz* secure definition, and the laws of the phenomenology of the memory image be formulated.

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EMOTIONS IN TERMS OF THE GALVANOMETRIC TECHNIQUE[1]

By CHRISTIAN A. RUCKMICK.
(*University of Iowa.*)

I. *Statement of the problem* (pp. 149–151).
II. *Summary of earlier results* (pp. 151–154).
III. *Experimental method* (pp. 154–155).
IV. *Discussion of results* (pp. 155–156).
V. *Suggested improvements in technique* (pp. 157–158).
VI. *Summary* (p. 159).

I. STATEMENT OF THE PROBLEM.

MOST investigators in the field of the affective processes are aware of the historical development of the expressive method in psychology. They know that the peculiar nature of these mental processes demands the use of two indirect procedures. While in our laboratory we have been successful so far in obtaining introspective accounts of some of the typical emotional experiences, reliance is also being placed on the registration of physiological effects that take place simultaneously. The fundamental reason for this stress seems to lie in the accepted fact that emotional experiences, more than any other, are accompanied by a considerable and widespread disturbance in the body. With this assumption we have rigorously to guard against the notion that these disturbances are themselves equivalent to the emotional experiences. The author has already protested against this interpretation, and has taken the stand that emotions are still, at least in part, liveable experiences in the human mind and probably also in the mind of other higher vertebrates[2].

The second indirect procedure is the impressive method usually developed in terms of affective judgments in regard to objects presented by the technique of paired comparisons. Since the latter method has developed quite independently of the former, we shall not discuss it here.

With the knowledge of the historical development of the physiological methods, careful research students will want to avoid the pitfalls which have beset previous investigations, involving various forms of bodily expression. It is altogether obvious that, heretofore, too much was expected from the graphic results thus obtained. Many of us suspected that, as the result of such procedures, the Wundtian tridimensional schema was all too neatly and too logically developed to withstand the stress of continued criticism[1]. The results of this investigation were not, even at that time, unequivocal. They suffered serious consequences, moreover, at the hands of subsequent workers. One of the largest sources of error lay in the fact that physiological processes are influenced by a variety of conditions outside of the affective responses. If we proceed cautiously we shall be able in time to factor out some of these conditions, but we certainly must not draw sweeping conclusions because the curves obtained seem to be so definite and so plausible.

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[1] Read at the fourth annual meeting of the Mid-western Psychological Association, Urbana, Illinois, May 10th, 1929.

[2] C. A. Ruckmick, "Why we have emotions," *Scient. Mo.* 1929, xxviii, 261.

As the physiological sciences have developed and physiological techniques have been provided, definite strides have been made in the direction of greater refinement of the method of expression. As the author has pointed out[2], it seems as though the psychologist has eagerly sought any new techniques which have been developed in physiology that seem promising for his own researches.

A great variety of physiological changes have been seized upon as offering an explanation of emotional expressions. Recently, with the common reduction of most physical phenomena to electrical changes and the great development in amplifying minute electrical discharges, there has come about a renewed faith in the galvanic reflex. The galvanometer has itself also been perfected and developed in the direction of photographic recording. Hathaway has furthermore developed a portable instrument which amplifies, through oscillating wave circuits, the output of electrical energy from the body[3]. One distinct advantage in most of these galvanic records is the appearance of the latent period which seems to mark off an affective, or what R. J. Bartlett terms an 'orectic,' response from muscular contractions *per se*[4]. Another advantage lies in the fact that this electrical energy is apparently not under the voluntary control of the observer.

A considerable amount of work has been done on the problem of the probable physiological cause for the electrical discharge. Clearly this is chiefly a problem for the physiologist himself. While psychologists are interested in the question and will do their share in arriving at a solution, it is not necessary to understand the mechanism of the galvanic reflex in order to obtain results significant for psychology. The writer is aware, of course, that some of the underlying conditions of the reflex affect the techniques, such as proper electrodal connections, but further experimental investigations in the psychological laboratory do not have to wait upon these fundamental considerations of the source of the electrical endosomatic changes before results can be announced.

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[1] N. Alechsieff, "Die Grundformen der Gefühle," *Psychol. Stud.* 1907, III, 156 ff.

[2] *Op. cit.* p. 260.

[3] Designed by S. R. Hathaway, Athens, Ohio.

[4] R. J. Bartlett, "Does the psychogalvanic phenomenon indicate emotion?" *Brit. J. Psychol.* 1927, XVIII, 30–50.

With this situation well in mind it is our purpose to summarize the chief results thus far obtained both by way of experimental data and in terms of improved technique. While credit is given, of course, to some of the outstanding achievements made in other laboratories, we hope to describe the status of emotion as developed in terms of research in our own laboratories at the University of Iowa. It will be understood that the material thus reviewed is subject to the usual scientific condition of further experimentation and criticism. While we have made more than two hundred introspective analyses of various types of emotions and moods together with the corresponding expressive galvanic records for many of them, we have made only a small beginning in this field of activity. Much remains to be done. If the progress so far has been slow we must not forget that much time, energy, and thought has gone into the arousal of real emotions and moods under laboratory conditions. The reports show that our observers have spontaneously attested to the reality of the emotions produced. It is the writer's intention to summarize, on a later occasion, the qualitative reports from our observers. The present paper will be devoted mainly to the galvanometric technique.

II. SUMMARY OF EARLIER RESULTS.

We may say first of all that the curves of deflection, especially those obtained from various types of sensitive galvanometers, show such typical characteristics for emotional processes that they can be easily distinguished on the whole from curves which seem to be due to general muscular contractions or to other physiological disturbances not directly correlated with the affective life. If careful records are kept of the incidence of the emotional stimulus or situation and the onset of the emotional processes in the observer, a typical initial latent period will invariably occur. This has been found by many other investigators in the field and lasts 5 to 12 seconds from the moment of arousal. Just what this latent period means is a physiological problem that has not yet been solved. It occurs also with the Hathaway apparatus but we have not yet graphically recorded it in the same way. The accompanying figure illustrates a typical instance.

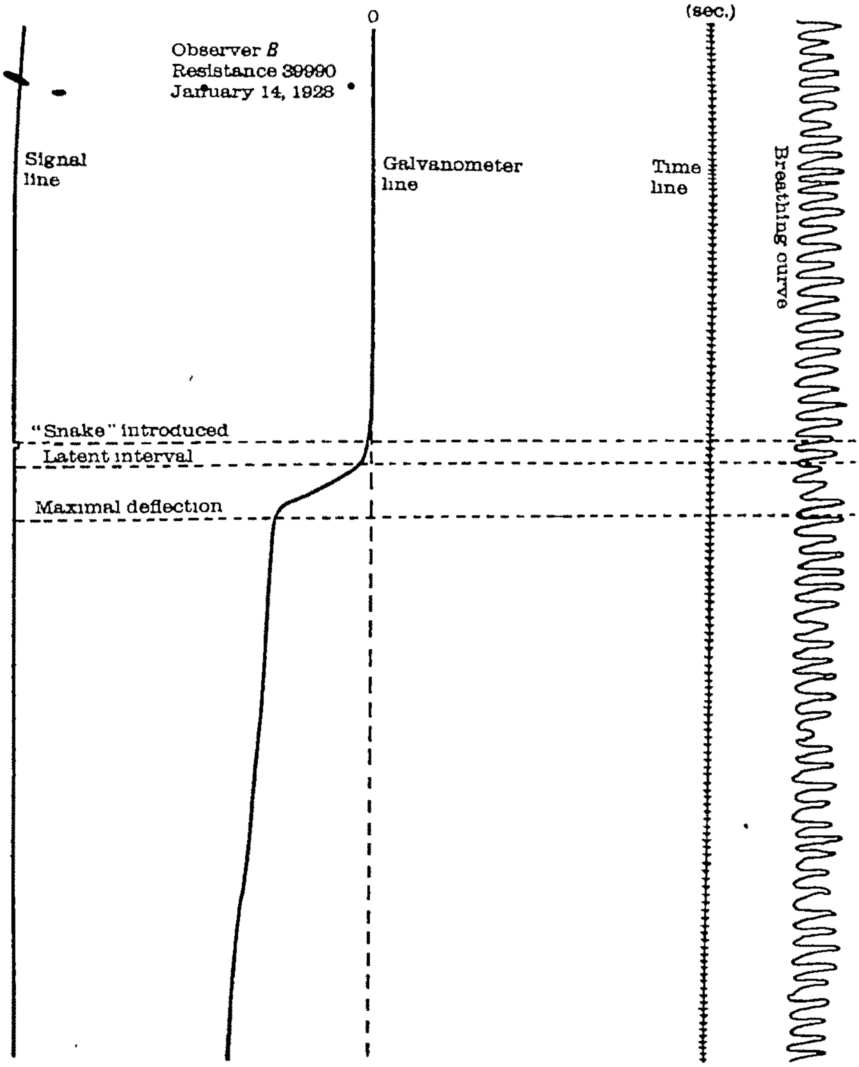

Fig. 1.

The second almost uniform characteristic of the curve is its initial, rapid change of direction. This movement in most of our records takes place to the left, indicating a very quick reduction of electrical resistance in the body. We are frank to confess, however, that in a few cases, which are so far anomalous, the deflection has occurred to the right under fairly identical conditions. As a rule, after the wide excursion at the beginning, there are then no further prominent deflections in the original direction. Gradually the curve turns to the zero line, sometimes with one or more prominent humps.

All of our studies carried on in many different directions so far clearly reveal the fact that there is a high general correlation between the introspectively reported intensity of the emotion and the amplitude of the deflection. It is to be understood, of course, that the observers are kept in ignorance of the records obtained. Under these circumstances it is rather significant to note that at the present stage of our knowledge we can directly indicate the approximate intensity of the emotional processes. A further qualification of this point will be made later in connection with the higher emotions or the sentiments, but no qualification need be entered for the primitive emotions.

In a preliminary way we can say that certain types of curves tally in form or contour with certain emotional experiences. This comparison has not succeeded nearly as well as we had anticipated, but Bayley has shown that two types of fear, namely, startle and apprehension, may be distinguished to a certain extent on this basis[1]. It is very likely that, when we continue studying these and similar emotions more closely, we shall find that the graphic record showing such differentiations depends upon inherent characteristics in the respective emotional experiences. These elements may be factored out through the introspective records. In this way our results indicate that some of the names for emotional states represent differences in intensity, in temporal course, and in ideational content, all in terms of its general pattern or scheme. But we have not enough data at present to clinch this conclusion.

Taking as our cue the historical fate of the expressive method when based upon respiration and circulation, we have also attempted to study the effect of some general bodily conditions as manifested through the so-called galvanic reflex. The experimental investigations made by Bayley and later by Patterson had already suggested that the total bodily resistance was affected by conditions of mental and physical fatigue[2]. The introspective records had previously given us hints in this direction through reports of alertness and euphoria and their opposites.

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[1] N. Bayley, "A study of fear by means of the psychogalvanic technique," *Psychol. Monog.* 1928, XXXVIII, No. 176, pp. 31–33 (Univ. of Iowa Stud. in Psychol. No. XI).

[2] N. Bayley, *op. cit.* pp. 26, 27; E. Patterson, "A qualitative and quantitative study of the emotion of surprise" (published in Univ. of Iowa Stud. in Psychol. No. XIII).

III. EXPERIMENTAL METHOD.

The writer therefore undertook a series of experiments in order to verify these cues. Seven observers were used for both the normal and the fatigue series, but the main series was finally conducted principally with two observers. The technique involved mental fatigue induced by long arithmetical problems and a series of searching questions in psychology and other tasks done under high pressure with threat of electrical shock or intensive scolding as punishment for failures made and the alternative of physical reward, such as refreshments, for successful solutions. Although there was a slight reduction of resistance in most cases, this series involving mental fatigue only did not indicate enough change of resistance to be significant. Consequently a series of experiments was undertaken to determine the effect of physical fatigue on bodily resistance. These trials were carried to the point of almost complete physical and mental exhaustion. The method involved the use of the algometer on the lips and the mucous membrane of the nose. Electric shocks as strong as could be stood were also used in the series. The main portion of the series consisted of rather strenuous setting-up exercises such as raising and lowering the body from a table in the horizontal position and continued running 'in place.'

Detailed reports were taken showing some very significant features of fatigue. The following are typical:

(B) Attention distracted by solution in mouth; choking. Smarting of eyes. Strain in muscles in arms and shoulders, like weight pressing down. Also in legs, especially thighs. Feeling of relaxation, of lassitude throughout. Smarting of eyelids continued with quivering of chin. Subvocal idea—"Heavens, I must be tired. I never cry when I am not." Pressure of back of chair on my back. Heaviness of head. Could hardly hold it up. Unpleasant but not decidedly so. Feeling of 'detachment.' Spasmodic contraction of muscles in chin, throat and lower lip. Combatted by setting teeth, holding jaw tense, *etc.*, but lack of control. General attitude—if crying came it wouldn't matter. Besides kinaesthetic sensations of feeling heavy. Half formulation ('way, way down') of answers to offending remarks (scolding). Vague, very vague resentment, but it did not matter much one way or another.

(D) Feeling tone indifferent. Kinaesthetic sensations from eyelids extremely heavy. Drawn in corners. Not easy to open. Lips felt parched. Pressure sensations from mouth which hung open. Lips felt thick. Ideational material very sparse. The same material but repeated and repeated. Words repeated slowly and rhythmically. No noticeable visual imagery present. Mind sterile of ideas.

(F) Quivering sensations. "I had a hard time keeping still before the camera. More 'doggone' tired than have been for long time." Eyelids closed with considerable pressure sensations in inside. Vague tendency toward dizziness. Top of head felt heavy with a sort of gradual 'squelching' pressure. Dry tactual sensations in lips and throat. Forehead felt pinched. Breathing very slow and measured, also pressure sensations across shoulders, but in terms of verbal ideas commented on fact that rest of body didn't seem directly tired, though could hardly move from top of table. Not pleasant or unpleasant. Sheer indifference.

IV. RESULTS.

The interesting feature of the results lies in the consistent decrease of bodily resistance during fatigue when this is measured by the galvanometer. Under the same conditions, however, the Hathaway apparatus shows increasing bodily resistance in many instances. We are not quite sure what this difference in the two types of recording fundamentally means. It is worth pointing out, however, that the Hathaway apparatus records changes in the flow of an alternating current while the galvano-

Table I. *Normal* v. *fatigue conditions.*

	Galvanometer				Hathaway			
O.	N.	F.	D.	±	N.	F.	D.	±
A	3500	2600	900	–	37	50	13	+
A	2600	900	1700	–	60	37	23	–
A	1600	900	700	–	32	39	7	+
B	4500	2600	1900	–	39	12·5	26·5	–
C	3300	2500	800	–	30	40	10	+
C	2500	1300	1200	–	43	38	5	–
D	2500	1500	1000	–	51	42	9	–
D	4000	3300	700	–	30	33	3	+
D	2200	1300	900	–	35	44	9	+
D	4100	2900	1200	–	40	47	7	+
D	1420	990	430	–				
D	1500	1300	200	–				
D	...	...	...	...	88	112	24	+
D	...	...	...	...	94	138	44	+
E	1150	1130	20	–				
F	2949	2620	329	–	143	160	17	+
F	2070	1550	520	–	31·7	36	4·3	+
F	3290	2230	1060	–	30	28	2	–
F	3100	2100	1000	–	41	47	6	+
F	4300	2600	1700	–	40	29·5	10·5	–
F	2510	2300	210	–				
F	9500	8100	1400	–				
F	3050	2610	440	–				
F	...	...	...	...	95	104	9	+
F	...	...	...	...	90	115	25	+
F	...	...	...	...	62·5	40	22·5	–
F	...	...	...	...	46	50	4	+
F	...	...	...	...	55	78	23	+
F	...	...	...	...	30	32	2	+
F	...	...	...	...	39	51	12	+
F	...	...	...	...	24·5	27	2·5	+
G	4200	2700	1500	–	36	37	1	+
G	4600	2400	2200	–	36	34	2	–
G	2800	2000	800	–	35	33	2	–
G	3500	2700	800	–				

O. observer. N. normal condition. F. fatigued condition. + increase of bodily electrical resistance. – decrease of bodily electrical resistance. Galvanometer readings in terms of units of 10 ohms. Hathaway readings are potentiometer units on an arbitrary scale.

meter registers changes in a direct current. It follows, therefore, that there may be a distinction between endosomatic and ectosomatic current. In the Hathaway apparatus the lowering of endosomatic current may show increased resistance to alternating pressures, but we are not sure. In those series in which both the galvanometer and the Hathaway apparatus were used simultaneously we again obtained opposite results. These results may throw some light on the underlying cause for the galvanic reflex. The above table indicates the resistance obtained under these conditions.

There was also a rather consistent lowering of resistance on the part of three observers in another series when the right arm was outstretched in a horizontal direction and kept as long as possible in that position. The following table will indicate these results at successive intervals.

Table II. *Right arm outstretched.*

(Wheatstone bridge settings with galvanometer at zero in units of 10 ohms.)

O.	N.	0″	2″	4″	6″	7″	8″	10″
E	1150	1130	1090	1070	1050	—	1040	1020
B	2520	2320	2140	2050	1600	1520		
A	900	900	830	810	810			

Some short series with a number of observers indicate also that bodily position changes the total resistance. Experiments show that resistance is lowered when the body is in a prone position as over against the usual sitting position. Further work needs to be done in these directions but the indications so far are clear.

Another factor which developed throughout our trials was the significance of gross bodily processes *v.* ideational contents. Emotions which had strong bodily accompaniments, such as muscular or organic sensations, uniformily gave us large deflections. Emotions which are rather poor in such processes but rich in ideas that became affectively coloured, gave us relative scant deflections. This had already been mentioned in the Bayley and Patterson studies and has been noted in a recent article by Wiersma[1]. It appears, therefore, that the galvanometer after all responds more definitely with emotional responses of the primitive sort and less with sentimental experiences or emotions with a considerable ideational background and not primarily of an instinctive sort.

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[1] E. D. Wiersma, "On the value of the simultaneous registration of the plethysmogram and the psychogalvanic reaction," *Kon. Akad. v. Wet. Amster.* 1915, vii, 18. For an excellent review of this point see C. W. Darrow, "Differences in the physiological reactions to sensory and ideational stimuli," *Psychol. Bull.* 1929, xxvi, 185–201.

V. SUGGESTED IMPROVEMENTS IN TECHNIQUE.

Some work which we have recently undertaken in connection with the playing of checkers or 'drafts' has brought out this difference very effectively. At the beginning of the game where introspections revealed considerable excitement in anticipation of the game with large bodily complexes involved, we usually found wide deflections of the milliammeter readings on the Hathaway apparatus. Later on, when the game became definitely intellectualized and the attention of the observer was directed to the solution of problems, the milliammeter settled down to

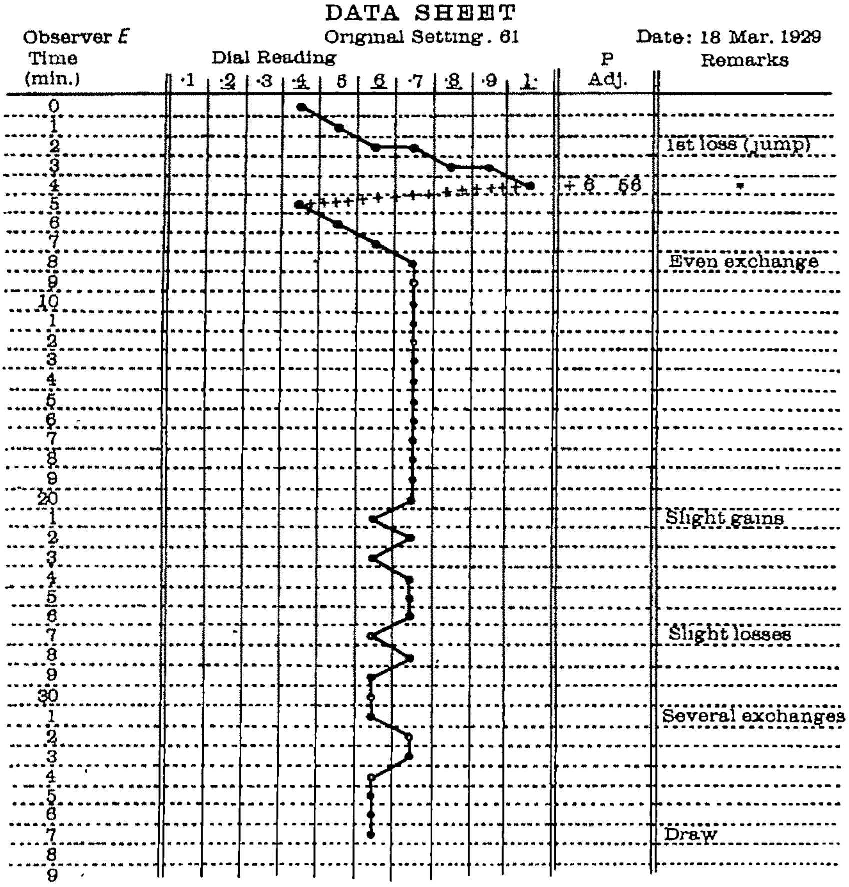

Fig. 2.

* When the milliammeter indicator of current passes off the scale it is brought back by readjusting the potentiometer.

a central position and stayed there with relatively little movement until the end of the game. This is shown in the accompanying diagram.

New improvements have also been made in the direction of better electrodal connections. We have already published descriptions of previous improvements[1]. But recently we have developed a band for the left hand which is particularly suitable for the Hathaway apparatus. The metallic contact is made through two plates made of very thin copper, 32 mm. square, stitched on fairly thin kid leather about 3 mm. apart. This leather forms a flap about 8 cm. long by 6 cm. wide which is stitched on to a longer band of about 15 cm. so that it leaves a free edge on one side. The longer band is furnished with two straps and two buckles and has a small piece cut out to fit around the thumb. Between the flap and the larger band a small piece of sponge rubber is inserted. Leads to the two copper plates are brought out, so that an ordinary lamp-cord with clip attachments can be connected to them. The other end of the lamp contains a standard radiophone plug.

When the band is strapped tightly to the left hand it is very comfortably worn and leaves the hand free for other work without disturbing the original pressure and area of contact. Of course the usual care in cleaning the copper plates and the palm of the hand with 95 per cent. ethyl alcohol should be exercised. After a preliminary period of about three minutes under normal conditions it will be found that the maximal degree of humidity from perspiration is reached. This will, therefore, vary but little throughout the experiment. The particular advantage of the contact in addition to the question of pressure and area lies in the comparative mobility of the hand over a long period of time, especially when the clips of the connecting cord are removed. The practised observer ordinarily is not distracted by wearing the electrode since it fits so comfortably that it virtually becomes part of his wearing apparel.

Another problem on which progress can be reported is the question of quick recording of results. A chart has been devised by means of which a rapid record can be taken of the deflections made by the pointer on the dial by the milliammeter. A specimen chart from a typical checker game has been shown above (Fig. 2).

We are still formulating plans for a photographic recording of the moving pointer, but for most of our situations the manual record is sufficient. Our most promising line of attack is to amplify the current still further so that it will actuate a moving optical lever.

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[1] C. A. Ruckmick and E. Patterson, "A simple non-polarizing electrode," *Amer. J. Psychol.* 1929, XLI, 120.

VI. SUMMARY.

By way of summary we may then conclude that the galvanic reflex, when properly safeguarded against both uncontrolled conditions within the body that are foreign to the emotional situation and the fortuitous factors in the electrodes, affords us a promising approach to a quantitative and graphic analysis of the emotional life. At the same time we have not overlooked the opportunity of training our observers to make careful analytical descriptions of their experiences during even the most intense emotional crises. The accounts by way of the method of expression are then made to run parallel with qualitative analyses. The writer believes that the main hope of the psychology of emotion, in the face of a rather troublesome past, lies in this direction. Our returns so far are necessarily scant and provisional, but we can say with some degree of certainty that we have a means of graphically recording the temporal course of an emotional process with its intensity closely corresponding to the amount of variation in bodily resistance. We may say further that the most primitive and instinctive emotions, *i.e.* those having large bodily components in terms of kinaesthesis or organic sensations, more definitely show graphic disturbances; emotions that are full of ideational material, on the other hand, give much more reduced deflections. In any case the disturbances accompanying emotional experiences can ordinarily be distinguished from bodily responses as a whole through the appearance of the initial latent period followed by a rapid incidence of the curve of deflection.

As soon as we have enough material to give assurance that the disturbing factors are brought under control we shall be in a position to pursue our research in many directions such as musical appreciation, dramatic performance, public speaking, and the effects of the cinematograph on children and adults. History teaches us, however, that we should proceed cautiously and at our own risk because the road to our goal is "under construction ahead" and is being "kept open only for the convenience of the [professional] public."

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A FURTHER ATTEMPT TO MEASURE THE STRENGTH OF INSTINCTS

By EMILY N. KENDREW.

(*Morley Memorial School, Cambridge.*)

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II. *First group* (pp. 161–168).
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  Results (pp. 169–172).
IV. *Comments and conclusions* (pp. 172–173).

I. INTRODUCTION.

SOME time ago an article appeared in *The Forum of Education* describing an experiment which attempted to measure the strength of instincts[1]. The subjects of the experiment were given a list of words calculated to arouse certain instincts, and were told to say at once whatever occurred to them on reading each word. The nature of the reply and the rate at which it followed the word-stimulus were supposed to indicate the intensity of the emotional response. At the same time, a psycho-galvanometer recorded any 'thrills' the subject might be experiencing. The emotional response secured by this double method was used as a basis for measuring the strength of the instinct stimulated.

Shortly after Dr A. C. Garnett wrote in this *Journal* on "A conative criterion for the discrimination of instincts[2]." Here he claimed that conation is the distinguishing mark of an instinct. But if this be so, *i.e.* if conation rather than emotion is to be the criterion by which instincts are discriminated—why not use the former rather than the latter as a basis in attempting to measure their strength?

A conative process, in other words, the type of behaviour aroused by an impulse and tending to continue until the impulse prompting it is satisfied, may include various forms of activity. Quite often, some of these involve skill. 'Skill' is used here to mean manipulative dexterity depending upon muscular control and the accuracy of visual perception.

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[1] Allman and McRae, "An attempt to measure the strength of instincts, Nov. 1927.
[2] Jan. 1928.

It is possible to compare degrees of skill and to measure any improvement which occurs. Improvement in skill is, generally, the result of practice, but experiments have suggested that incentives may bring about a similar result. An incentive, presumably, makes the hand more cunning and, like love, "adds a precious seeing to the eye." If, at the same time, the incentive is calculated to arouse some instinct, may not the improvement shown bear some relation to the strength of the instinct aroused? Further, supposing the improvement in skill is accompanied by alteration in speed, for example, or by other changes indicating increase of interest or effort, may not these changes afford further evidence which can be used in attempting to measure the strength of instincts?

On the other hand, increased effort if tinged with anxiety will probably impair skill, but this fact does not weaken the above hypothesis, for behaviour expressing anxiety, although it may lead to inefficiency, may also be taken as indicating the strength of the instinct aroused.

The following experiment is an attempt to work out the idea of using a conative basis for the measurement of instincts. The test used is a simple one: piling up dominoes one upon the other. The height of the pile is taken as representing the level of skill. (This does not mean that it represents only skill; other qualities related to Temperament and Character are also involved, but no attempt is made here to consider them.)

II. FIRST GROUP.

Subjects. Eight children from the Demonstration School, Homerton College. (Seven girls and one boy.)

	Age		Mental age*		
	Years	Months	Years	Months	I.Q.*
Freda	6	2	6	6	105
Clare	6	2	6	4	102
Margaret	5	4	5	2	98
Hugh	5	2	5	4	103
Jean	5	0	5	8	115
Marjory	4	2	5	7	134
Ruby	4	2	3	8	88
Evelyn	3	10	5	6	145

* According to the Terman revision of the Binet-Simon tests.

Incentives. Showing the order in which they were applied: Competition; Food-seeking; Curiosity; Self-competition.

(*a*) *Description of experiment.*

The children were tested separately. The dominoes were put in a shallow box at an easy distance from the child's right hand. At the beginning of the first test the child was told to "put the dominoes one on top of another, like this (one was laid flat on the table) and see how big a pile you can get." Five piles were built in each test; every child had seven tests, separated by at least one day's interval. The first three were for practice; no suggestions as to the best way to pile up the dominoes were given, but by the end of these each child had developed its own method of building, which was employed with very few variations during the rest of the experiment. Then followed the four incentive tests in the order given.

The results of the practices will be compared with those of the incentive tests later.

The incentive tests were conducted in the following way.

Competition. Two children worked together. Frequent suggestions were made that each should try to beat the other by getting a higher pile.

Food-seeking. The child was shown a plate of walnut macaroons of various colours and allowed to choose one. This was placed on the top of a column of bricks in front of her. The column was a little higher than the highest pile she had built as yet. She was told that she was to have the macaroon if she made a pile that would reach it.

Curiosity. A parcel containing a toy xylophone was suspended over the table at which the child worked, at a similar height to that used for the previous test. She was led in blindfold, placed in the chair and her eyes uncovered. If she could build up to the parcel she might open it! During this test, several attempts were made by the experimenter to reinforce the stimulus. After the first trial the visit to the toy-stores and the difficulty of finding something really wonderful was described. After the second the anticipated pleasure of opening the parcel was discussed in great detail. After the third the child was allowed three guesses as to what was in the parcel, and when she began again after the fourth she was told this was her last chance.

Self-competition. Here an average was taken of all the child's previous results. A red column, representing the height of this was marked on a strip of white paper and pinned up in front of her. She was told she had already built as high as the red part, and was asked to see if she could get above it. Any increase was to be marked on the strip of paper and coloured in by the child herself.

(b) *Results.*

The figures represent the number of dominoes in a pile.

Table I. *Last practice test.*

	Trials					Average
	1st	2nd	3rd	4th	5th	score
Freda	25	19	19	25	21	21·8
Clare	22	20	19	18	23	20·4
Margaret	17	12	20	26	10	17
Hugh	27	28	30	20	26	26·2
Jean	30	10	18	19	14	18 2
Marjory	16	11	22	9	21	15 8
Ruby	15	7	11	5	5	8·6
Evelyn	18	19	16	4	25	16·4
					Average	18·05

Table II. *Competition.*

	Trials					Average
	1st	2nd	3rd	4th	5th	score
Freda	15	20	21	18	14	17 6
Clare	30	20	15	23	14	20·4
Margaret	17	20	24	18	25	21·8
Hugh	24	15	27	17	23	21·2
Hugh	30	27	20	20	30	25·4
Jean	14	22	27	23	18	20 8
Marjory	32	23	16	18	13	20·4
Ruby	13	16	6	9	22	13·2
Marjory	19	18	17	15	14	16·6
Evelyn	22	19	11	24	12	17·6
					Average	19·5

Table III. *Food.*

	Trials					Average
	1st	2nd	3rd	4th	5th	score
Freda	30	20	15	27	26	23·6
Clare	23	19	20	15	15	18·4
Margaret	21	35	18	19	28	24·2
Hugh	28	30	30	22	23	26·6
Jean	20	22	19	27	30	23·6
Marjory	20	21	27	24	15	21·1
Ruby	9	20	23	25	16	18·6
Evelyn	26	19	21	27	25	23·6
					Average	22·5

Table IV. *Curiosity.*

	Trials					Average
	1st	2nd	3rd	4th	5th	score
Freda	20	24	21	22	28	23
Clare	24	21	22	15	18	20
Margaret	24	30	17	22	18	22·2
Hugh	22	30	30	26	27	27
Jean	17	30	30	24	32	26·6
Marjory	33	14	25	22	28	24·4
Ruby	8	12	18	12	17	13·4
Evelyn	21	24	20	22	14	20·2
					Average	22·1

Table V. *Self-competition.*

	Trials					Average score
	1st	2nd	3rd	4th	5th	
Freda	23	24	21	29	17	22·8
Clare	19	18	18	23	24	20·4
Margaret	28	21	26	25	27	25·4
Hugh (absent)	—	—	—	—	—	—
Jean	20	12	18	23	21	18·8
Marjory	26	19	23	28	28	24·8
Ruby	13	19	25	12	13	16·4
Evelyn	22	21	19	11	19	18·4
					Average	21

Table VI. *Showing the average for each child in each test.*

	Practice	Competition	Food-seeking	Curiosity	Self-competition
Freda	21·8	17·6	23·6	23	22·8
Clare	20·4	20·4	18·4	20	20·4
Margaret	17	21·8	24·2	22·2	25·4
Hugh	26·2	21·2 25·4	26·6	27	—
Jean	18·2	20·8	23·6	26·6	18·8
Marjory	15·8	20·4 16·6	21·1	24·4	24·8
Ruby	8·6	13·2	18·6	13·4	16·4
Evelyn	16·4	17·6	23·6	20·2	18·4

Table VII. *Giving the average score for each incentive and the highest pile built.*

	Average score	Highest pile
Practice test	18	30
Competition	19·5	32
Food-seeking	22·5	35
Curiosity	22·1	33
Self-competition	21	29

The scores shown in Table VII might possibly be dismissed as follows: increase up to food test—improvement due to practice, followed by decrease of score due to boredom! But further work with more children and the incentives taken in varying order is necessary before it is at all possible to estimate the significance of practice and boredom respectively. Table VII also shows that the variations in skill from test to test as indicated by the average scores made are very slight, but the significance of the variation in average score is borne out by the variation in the behaviour of the children during the tests. The following notes help to bring this out.

(*c*) *Notes on children's behaviour.*

Food-seeking. It will be seen that the highest average score and the highest actual pile were both reached during this test, which also showed less variation between the averages scores of the children: three had an average of 23, two had one of 18.

Notes on behaviour taken during the test show that many of the small scores made during this test may be accounted for by anxiety; as when Marjory at the beginning of her fifth attempt—which resulted in a pile of 15—gave a great sigh on being told she had just one more try, seized a handful of dominoes, knocked two off her little pile, jerked a few more on and gave a general impression of agitation; or, as in Clare's case, when her hand was trembling during the last two trials which made 15 and 15 respectively. Freda during the 20 and 15 trials could not leave her pile alone, but adjusted and readjusted the whole of it each time she added another domino. Hurried movements, shakiness and fussiness, although they retard skill, are nevertheless signs of effort and therefore have value as evidence in this experiment.

With a few exceptions which will be mentioned shortly, the behaviour of the children generally throughout the whole of the test expressed interest and effort. While they were busy with the preliminary practices there was a considerable amount of irrelevant talk: Evelyn gave an account of her mother's visit to a jumble sale; Margaret reminded me of our meeting on the bus on the previous day, and later described the various dainties she had brought with her for lunch: "Got an orange for my lunch and a piece of choc.," etc., but in face of the food incentive very little was said and every remark was related to the business in hand. Many of them ran something like this: "Nearly up to it now"; "I'm getting near" (in a whisper); "If I get up there!" (pointing to the goal); "Got it a bit crooked"; "Not coming down yet"; "Look how big the thing is"; and Jean exhorted her pile to "Keep up, Mister, Oh, do keep up or I shan't get my sweetie. Don't fall down yet." Ruby's interest was very evident. During her practices she had said: "Wish I wasn't to do them"; "Wish I went off now"; "Can I go?"; and on being told that the next trial was her last she deliberately got a handful of dominoes, dropped them on her pile, and when it fell on reaching the height of five announced "I've done it," *i.e.* finished my last pile. But this time, when she had finished the test and not got her macaroon, she looked at me with: "You said that was the last one. Is there *only* the last one?" The children looked constantly at the goal—now and again with a sigh—some of them even kept their eyes fixed on it while they reached for more dominoes, and one little girl got her pile partly up before she could take her eyes off it. This last mode of behaviour may be contrasted with a certain amount of gazing round which took place during the practices.

Many examples could be given of how the incentive aroused the children to greater effort and to more careful work; one placed her pile well back on the table and eyed it carefully before adjusting any irregularities; Clare bent her head so that her eyes were level with each domino as she placed it; Freda held each one with both hands over the pile and steadily lowered it, while Ruby held on to the bottom of her pile with the left hand in a—usually vain—attempt to keep it up till it was really high. Every child placed carefully and made some attempt at adjusting.

Some changes in speed were noticeable. In one practice test Evelyn had spent 3 min. 50 sec. on a pile of 25—her excursions into regions remote have already been mentioned, and they took time!—but during this test she was never longer than 1 min. 50 sec., whereas Marjory, because of her greater carefulness, now spent from 1 min. 30 sec. to 2 min. on each pile instead of always being under the minute as previously. Why the same stimulus should act with one child as an accelerator and with another as a brake may be explained as follows. In the former case the interest which has been aroused is strong enough to hold its own against all intruders which would claim attention and so waste time; in the latter the desire to reach the goal makes the child more careful and consequently more deliberate. That the children react in opposite ways to the same incentive is evidence of temperamental differences between them.

The exceptions to the general type of behaviour illustrated above were the cases of Margaret and Hugh. The former had her turn late in the morning, and after her second trial heard the children in the cloak-room preparing for home. She began her next trial with the remark, "They're going home now, ain't they?"; and her third and fourth scores were 18 and 19 as against 35 for her second. She pulled herself together for the fifth after I had impressed upon her that it was her last chance and reached a score of 28. Hugh, after his third trial, became rather indifferent and began to talk: "Miss Kendrew, I've got a book at home of lions and bears. I'll bring it to school," etc., etc., with the result that his scores fell from 30, 30 to 22, 23.

Curiosity. The numerical difference between the results of the Food-seeking and Curiosity tests is very small, but nevertheless significant, since the decreased score is accompanied by behaviour showing less interest and giving fewer examples of effort. Freda placed rather carelessly and ignored a curve which brought her pile down at 21. Clare scarcely looked at the object, and Margaret stared round and did not trouble to adjust. Evelyn showed a tendency to go back to her dilatory practice-test ways; she played with the dominoes before placing them:

put a handful into her lap and took from there and the box alternately and so managed to spend 2½ min. on a pile of 20. In her last trial she took no notice of irregularities at the base of her pile, with the result that the score was only 14. These are some examples of behaviour which may be contrasted with those given in the Food-seeking test notes.

The improvement shown in Clare's score and in Marjory's was probably due to the absence of nervousness—as contrasted with the definite signs of anxiety mentioned in the Food-seeking test notes. If, as suggested in the introduction, the degree of anxiety bears some relation to the strength of the instinct, the higher scores do not, in this case, mean that the incentive was more powerful.

Self-competition. The average for this test shows a further decrease. This again is accompanied by behaviour which appears to be significant. There are very few examples of behaviour showing interest and effort: Jean at the beginning of her second trial said, "See if I can get it up!"; and Evelyn ran her finger up her chart, then on as high as she could reach, saying, "I'll get it up as big as...." Marjory worked carefully and slowly, pursed her lips, looked constantly at her chart and began her last pile nearer to the pile of bricks. On the other hand there were a number of instances of indifference. Freda very rarely adjusted her piles—although each one was either twisted or irregular. Jean also ignored twists, did a fair amount of gazing round, and played with her dominoes before placing them. Ruby seemed to consider that some further incentive was necessary, for after I had explained to her about filling in any advance on the chart she asked, "and what then?" and remarked during her second trial, "I might get something" and "what will you show me?"—which suggests that the previous incentives were still active.

Margaret's improved score must be mentioned. For the first time she went through a test without speaking! During the Curiosity test she had kept up a constant chatter. "It's a doll"; "Is it dominoes?"; "Is it a nice thing?"; "Do you know what it is?" etc., etc., but in spite of this apparent interest and the fact that she was not disturbed as in the previous test, her average was lower than for that one. Margaret is a moody child. The former tests were carried out on two of her excitable days. The excitement was not in the first place aroused by the incentive, although it was probably increased by it. This last test was given when she was in a quiet, busy mood, hence the change in her achievement and behaviour.

Competition. It will be seen that the lowest average score was made during the Competition tests. The behaviour of the children this time can hardly be compared with that of any other test. A companion to work with made the conditions different, and the result was very much more conversation and relatively little serious effort. The tendency to be diverted from the main purpose because of the presence of a fellow-worker was shown by Freda saying to Clare as they began working: "Clare, how old are you?"; and later Clare remarked: "Someone's coming to have tea with me." That the incentive was not working as intended was shown by Margaret saying to Hugh after their first trial: "I'll try to get all mine up and you try to get all yours up." Marjory also when her last pile had fallen over went over to help Evelyn "to get all hers on," handing the dominoes to her and asking to be allowed to put some on. When, in spite of Marjory's assistance (or because of it) Evelyn's pile fell at 12, and they were told that Marjory's pile was the higher, Evelyn announced in a satisfied and admiring voice: "Coo, she's beated the game!"

Having in mind some theories bearing on infant sexuality, an attempt was made while Jean and Hugh were being tested to reinforce the competitive instinct by an appeal to sex. They were asked whether little girls or little boys should get the bigger pile. This led to the following conversation—which an earnest Freudian might have found somewhat disconcerting: Hugh to Jean, "You'd be a little boy if your hair was like mine"; Jean to Hugh, "If you'd my jumper on you'd be a little girl"—and to a drop of 4 in Jean's score, while Hugh just maintained his previous low score of 20 as against 30, 27 and 30 in other trials.

With two groups there developed a little game of pretending to shake the table so that both piles might fall, and Jean seemed to think that the best part of the business was seeing how soon a pile came down. As a rule, both children laughed heartily whenever one collapsed! Here we have another motive taking the place of the one the incentive was calculated to arouse: the play impulse was awakened, and the companion, instead of being a competitor became a playmate. With Freda, also a new motive appeared. Before her second trial she said: "I'm going to get mine nice." This remark suggests that some impulse associated with the self-regarding sentiment had appeared. These two examples are the only definite instances in the whole of the experiment of the original incentive being replaced by another.

In the conversation there was plenty of reiteration of idea based on the opening injunction: "See who can get the biggest pile!" such as "Wish I could get the biggest!"; "I've got the biggest," etc., etc., but most of it seemed like playing with words—for the remarks were thrown backwards and forwards—and good-humoured chatter.

III. SECOND GROUP.

Some months later the experiment was repeated with a different group from the same school whose ages corresponded as nearly as possible with those of the former children:

	Age	
	Years	Months
Dorothy	6	1
Marjorie	6	1
Dennis	6	1
Peter	5	1
Betty	5	0
Lewis	5	0
Eric	4	6
Olive	4	5
Hilary	4	5

One or two slight alterations in method were made: the children were told to place the dominoes, numbers downward, on the pile, one by one; and before the Food-seeking test they chose their own incentive from a bowl of apples, bananas and chocolate biscuits.

The Intelligence tests and the Self-competition test had to be omitted: there was not time for them.

Results.

Table I A. *Last practice test.*

	Trials					
	1st	2nd	3rd	4th	5th	Average score
Dorothy	15	35	22	33	22	25·4
Marjorie	24	16	32	23	20	23
Dennis	20	22	24	21	29	23·2
Peter	20	30	17	16	20	20·6
Betty	8	16	7	16	19	13·2
Lewis	7	21	15	12	19	14·8
Eric	19	21	22	13	32	21·4
Olive	23	23	20	20	24	22
Hilary	8	7	8	16	12	10·2
					Average	19·3

Table II A. *Food.*

	Trials					
	1st	2nd	3rd	4th	5th	Average score
Dorothy	22	37	34	41	38	34·4
Marjorie	25	23	22	22	23	23
Dennis	—	—	—	—	—	—
Peter	33	27	27	32	22	29·8
Betty	31	14	17	14	22	19·6
Lewis	17	30	28	22	25	26·2
Eric	19	17	14	33	12	19
Olive	28	34	29	36	38	33
Hilary	15	18	16	19	11	15·8
					Average	25·1

Table III A. *Competition.*

	Trials					Average score
	1st	2nd	3rd	4th	5th	
Dorothy	33	33	28	19	27	28
Marjorie	33	41	24	21	18	27·4
Dennis	21	26	16	36	14	22·6
Peter	26	25	36	14	26	25·4
Betty	16	22	24	26	27	23
Lewis	34	18	19	20	21	22·4
Eric	29	20	21	32	35	27·4
Olive	21	15	19	27	23	21
Hilary	11	20	14	4	6	11
					Average	23·1

Table IV A. *Curiosity.*

	Trials					Average score
	1st	2nd	3rd	4th	5th	
Dorothy	30	28	26	15	25	24·8
Marjorie	23	39	25	29	29	29
Dennis	29	23	31	35	27	29
Peter	18	20	27	14	8	19·8
Betty	26	25	12	16	27	21·2
Lewis	26	34	19	23	20	24·4
Eric (absent)	—	—	—	—	—	—
Olive	22	29	30	24	30	27
Hilary	18	15	15	9	12	13·8
					Average	23·6

Table V A. *Showing the average for each child in each test.*

	Practice	Food-seeking	Competition	Curiosity
Dorothy	25·4	34·4	28	24·8
Marjorie	23	23	27·4	29
Dennis	23·2	—	22·6	29
Peter	20·6	29·8	25·4	19·8
Betty	13·2	19·6	23	21·2
Lewis	14·8	26·2	22·4	24·4
Eric	21·4	19	27·4	—
Olive	22	33	21	27
Hilary	10·2	15·8	11	13·8

Table VI A. *Giving the average score for each incentive and the highest average during the test.*

	Average score	Highest average
Practice test	19·3	25·4
Food-seeking	25·1	34·4
Competition	23·1	28
Curiosity	23·6	29

So that a further comparison might be made, all scores for the incentive tests which were above the child's average last-practice score were weighted. To the consecutively increasing numbers in the pile were added 0·1, 0·2, 0·3, etc. (one to each) + the previous weight: *e.g.* if 25 = average last-practice score, 0·1 was added to 26; 0·2 + 0·1 to 27; 0·3 + 0·3 to 28, etc.

Table VII A. *Showing the weighted average for each child in each test.*

	Food-seeking	Competition	Curiosity
Dorothy	41·4	29·6	25·2
Marjorie	23·1	31·5	32·6
Dennis	—	24·5	31·9
Peter	35·4	29·3	20·5
Betty	24	29·3	26·8
Lewis	34·8	27·9	31·6
Eric	20·6	31·5	—
Olive	40·4	21·3	29·1
Hilary	18·2	12·3	15·2

Table VIII A. *Showing the weighted average score for each incentive and the highest weighted average during test.*

	Weighted average score	Highest weighted average
Food-seeking	29·7	41·4
Competition	26·3	31·5
Curiosity	26·6	32·6

If the numerical results of the two parts of the experiment are compared it will be seen that the general level of skill, as represented by the height of the piles, was, on the whole, higher the second time than the first. The instructions given at the beginning of the second series of tests may account for this, yet it seems probable that some children in the second group possessed greater constructive ability than any in the first.

A comparison of Tables VII, VI A and VIII A shows some degree of correspondence between them: the order of the incentives, as based on the numerical results, remains the same.

A number of examples were given in the first part of this paper to show the relation between behaviour and score: similar ones could be taken from the more recent tests if it were necessary.

Eric's work, however, may be noticed. During the Food-seeking test he had no score between 19 and 33; during Competition nothing between 21 and 29. The reason for this could be clearly traced in his behaviour. Each pile was persistently patted between his palms in its early stages. If, in spite of this treatment, it managed to maintain its equilibrium until it reached the height of about 20 or 22 (and only the fourth pile in the Food-seeking test accomplished this) Eric's zeal for adjusting suddenly expired, and he tried—very successfully, as his higher scores show—to keep the balance by careful placing only. If his results are omitted the weighted average scores in Table VIII A would be:

Food-seeking	31·4
Competition	25·7
Curiosity	26·6

IV. COMMENTS AND CONCLUSIONS.

1. The examples of behaviour seem to confirm such inferences as may reasonably be based on the numerical results, for in each test a change in the level of performance was accompanied by a corresponding change in the amount of interest or effort shown by the children.

2. Although the Food-seeking test shows a very slight advantage over the others in its average score, the significance of it is emphasized by the following points:

(*a*) It was the only test in which nervousness and outside distractions evidently lowered the score.

(*b*) Examples of behaviour expressing interest and effort were more numerous than in any of the other tests.

(*c*) There were no instances of behaviour showing lack of interest.

3. It must be remembered that before each trial in the Curiosity test an attempt was made to reinforce the strength of the stimulus. This must be regarded as an advantage when this test is compared with the others. Also after each trial in the Competition test one of the two children had the extra stimulus of a temporary success, whereas in the Food-seeking and Curiosity tests each trial ended in the disappointment and probable discouragement of failure.

4. In considering the results of Competition, the low score and lack of behaviour expressing effort is accounted for by the fact that the stimulus, *i.e.* the other child, was regarded as a companion, not as an opponent. This suggests either that the competitive instinct is weak in young children, or that the methods of this particular school (which encourage the children to take an interest in each other's work) do not develop it.

5. Alterations in the height of the piles were not due to changed method for, in the first part of the experiment, each child had developed her own and continued to use it, while in the second part a uniform method was enforced.

6. Individual variety of method employed in building has been mentioned. One child put on the dominoes singly, adjusting each one

if necessary; another arranged three or four carefully in her hand and then placed them, while a third put an irregular handful on the pile and adjusted them there; Hugh made a small pile on the table, regulated it and then transferred it to his main one; and so on.

7. Another feature of the experiment which is more difficult to illustrate was the variety in the general type of behaviour of one child as compared with another. This variety was, probably, largely the result of differences in temperament. When this variety, and the pronounced differences in the method of working, together with differences in age and intelligence are realized, it seems all the more significant that any group reaction can be noted in the course of the experiment. This latter suggests that the incentive aroused some conative impulse common to the group of children, and that the ensuing behaviour gives some indication of its strength.

8. Reference has been made to temperament. The children were too young to be given the June Downey will-temperament tests, which is unfortunate, for it seems probable that temperament played a more important part in causing variations from the average standard than did either age or intelligence. Experiments might profitably be carried out to show the effect of incentives upon varying temperaments, and the relation of temperament to instinct.

9. Finally, although the experiment was only carried out on a very small scale, there seems to be some ground for suggesting that further work may be done on the relation of achievement and behaviour to the strength of instincts.

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THE BIOLOGICAL SIGNIFICANCE OF BLUSHING AND SHAME.

By JOHN T. MACCURDY.

(*From the Psychological Laboratory, Cambridge.*)

During the past century there has appeared intermittently a series of publications on the topics of this brief contribution. They were brought to a focus by Darwin in his *Expressions of Emotions in Man and Animals*, and again by Havelock Ellis in his monumental *Studies in the Psychology of Sex*. That these—and other—authors failed to achieve a satisfactory and unitary theory was due, I believe, primarily to their ignorance of the modern doctrine of the antagonism of the sympathetic and vagal divisions of the involuntary nervous system, and secondarily to their neglect of concealment and immobility behaviour as a reaction to danger. This neglect is no longer possible, since Rivers has advertised the fact that fear-flight and anger-aggression are by no means the only primitive reactions to danger. Cannon[1], with delicate technique, has shown how the emotions of fear and rage are accompanied by over-activity of the sympathetic-adrenal system, and his work has been cited with dreary iteration by all those who uphold the James-Lange theory of emotions, without their realizing that—so far as Cannon's findings go—it would seem to indicate that the physiological basis is the same for all emotions. It is my intention to indicate how in blushing and shame we may have an emotion with a different physiology and one that is part of a much neglected, though important, biological reaction to danger.

According to the view of Darwin, which impregnates modern psychology, the expressions of emotion are instinctive reactions that were adaptive in a pre-human or early savage period, even if they are no longer useful; or they are parts of general adaptive physiological disturbances that have survived because they have attained some social significance. In this sense he discusses not only involuntary gestures and grimaces, but also changes in circulation, secretion and so forth. The latter, being normally under no mental control, have the deepest biological significance, and Cannon has argued that sympathetic-adrenal stimulation betokens a response to undue physical exertion, originally consequent thereto but now, as an emotional reaction, prospective and preparatory. There seems to be no reason why this interpretation should be rejected and, if we accept it, the converse should also hold, namely, that if an instinctive response is towards immobility, its involuntary accompaniments ought to represent the opposite swing of the vagal-sympathetic pendulum, that is, a paralysis of the sympathetic and a stimulation of the vagal system.

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[1] *Bodily changes in Pain, Hunger, Fear and Rage*, New York, 1915.

The careless observer is apt to think that blushing is a reaction confined to the face, ears and neck. All who have studied the phenomena agree, however, that subjectively, at least, the disturbance has a wider distribution than is objectively manifest. The most frequent report is of the skin of the trunk and sometimes of the whole body, feeling hot or even tingling and pricking. As long ago as 1839 Burgess[1] opined that there was a tendency for all the skin to redden, but that it did so usually only on those surfaces which are habitually exposed and where the minute blood vessels have become more labile than in protected regions. This view is now confirmed by Lewis in the course of his prolonged and critical study[2] of vascular skin reactions, in which he observed that exposure produces atonia and consequent liability to dilatation in superficial blood vessels. The boundary of a woman's blush is normally the same as that reddened or tanned in the sun and varies with the fashions of the age. Amyl nitrite produces a flush in the same area, and Lewis thinks that emotional blushing may be due to the appearance of a substance which affects the peripheral vessels in an analogous manner. Its appearance is too rapid, however, to be the result of a chemical distributed through the blood stream and, if it be produced *in situ*, it must be in consequence of a specific nervous stimulus. It is tempting to assume that the nervous influence is an upsetting of the normal balance between the vagal and sympathetic nerve supplies. (Stimulation of the sympathetic produces pallor, and section of a sympathetic nerve gives flushing.)

This assumption receives some support from the work of G. E. Partridge[3], who examined the answers of some 120 subjects to a questionnaire about blushing, promulgated by Stanley Hall. Unfortunately his observers were not expert at discriminating between neurotic and normal manifestations, and there are no objective phenomena reported. Moreover Partridge jumbles all the findings together, so that rather contradictory reactions appear in the same summary. One does not know how many of the events described occurred in one case. The total list includes queer sensations from all parts of the body, and some of these suggest sympathetic rather than vagal stimulation. But, unless the order of symptoms is known and the sympathetic response is seen to be primary, there is nothing in this to upset the view that blushing is an external manifestation of a widespread vagal display. In the re-establishment of balance the body often goes from one extreme to the other. We know that a strengthening of the vagal over the sympathetic component leads to a fall of blood pressure and a slowing of the heart: it is like a shock. Partridge says: "Some...effects are undoubtedly due to the momentary cessation of the heart beat, there is a temporary lowering of general pressure and anaemia of peripheral blood vessels, then a sudden renewal of heart action and increased pressure." One of his conclusions is that, "The shock element plays an important part in the causes of blushing....There is a 'caught in the act' feeling about it."

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[1] *The Physiology or Mechanism of Blushing*, 1839, pp. 114, 122.

[2] *The Blood Vessels of the Human Skin and Their Responses*, London, 1927, p. 157.

[3] "Blushing" Pedagogical Seminary, April 1927.

A crucial phenomenon would be a contraction of the pupils coincident with blushing, as it would show that the involuntary nervous system balance was upset in a vagal direction. Unfortunately this is a difficult observation to make: the illumination of the eyes must be constant as well as the distance adaptation of the iris muscles, the circumstances under which blushing occurs are those in which it is impolite to gaze into the eyes of the subject and, finally, with blushing the subject almost always looks down or avoids a glance. A neurologist friend of mine who thinks the pupils do contract has been trying for months to make a definite observation but has so far failed to see anything one way or another.

It has been suggested above that, if sympathetic action has the biological significance of muscular exertion, a vagal preponderance should indicate the reverse tendency—that of immobility. Since most, if not all, animals have an enormously high visual threshold for motionless objects for which they are not searching, the immobility reaction affords a security that is difficult for us humans to realize. But it depends on its completeness: a tremor, a movement of the eyeballs will betray it. Hence the threshold for responses to the environment must be raised as it is in human sleep or stupor. In these states the involuntary nervous system changes are indicative of reduced sympathetic activity, so it is not straining probability to assume that during immobility there is a similar fall in blood pressure and general inertness due to sympathetic paresis.

The behaviour displayed in the assumption and maintenance of immobility is so simple as almost to be called reflex. Its object is concealment, which man achieves through highly complicated instinctive, or consciously planned behaviour. He hides himself and does not trust to nature's cloak of invisibility; it is safer to be behind an opaque rock than to trust to an enemy's failure to see the man in front of the rock as only a part of the latter. The complicated reaction having given greater security the more primitive one lapses and reappears only as an abnormal response or in fragments in the structure of an emotion. James said that we are afraid because we run. A more accurate statement of his theory would be: we are afraid because part of us wants to run; that part (the sympathetic system) is running wild and gives the feeling of fear or, at least, contributes to it. The inner man of the prisoner, who pleads his case pale, sweating and with pounding heart, is in flight while the outer man is still. Part of him is indulging in a response appropriate only to more primitive behaviour. We should similarly expect a sudden desire for concealment to be accompanied by some evidence of the immobility reaction. Blushing and the feelings going with fall of blood pressure are an indication of this.

The association of shame with a tendency towards concealment has been stressed by many authors from Darwin onwards, while its relationship to danger was noted even by the ancients. The mistake has usually been made of associating shame with fear on the naïve view that flight is the only response to danger. Darwin, being a biologist, was not guilty of that error, and, being a psychologist as well, he knew that different emotions must have different origins. The unhappy wretch who blushes, averts his eyes, hangs his head, covers his face with his hands and "wishes he might sink through the floor," is hardly the picture of fear. Moreover the mental confusion so common in embarrassment is more characteristic of the inertness of immobility than of the liveliness of flight. In fact the common phrase 'covered with confusion' contains both the elements of concealment and incapacity to respond. Among etymologists the favourite theory for the origin of the word 'shame' is that it comes from a preteutonic root cognate with a word meaning to cover.

Those who have claimed that embarrassment is associated with danger have arrived at their conclusion from a study of all the component phenomena which they interpret as impulsive efforts to avoid the observation of hostile onlookers. Yet it is notorious that in our society the situations which excite shame do not involve any physical threat. This is a far cry from a primitive immobility reaction and it challenges explanation. The common occasions for our embarrassment are exposure of the parts of the body usually hidden, the betrayal of sexual thoughts, lapses of etiquette and unusual conspicuousness of the subject. What kind of evolution could give these predicaments a meaning of danger? The answer is to be found in a study of shame as it occurs among savages and in a consideration of the development of modesty.

A most important point to bear in mind is that immobility or concealment is not a reaction to an already existent danger, but aims at preventing the danger from arising. If I am perfectly concealed I need fear no foe, no matter how near he may be. Concealment is therefore sought prior to the performance of any act which would preclude rapid self-defence. In general the indulgence of any appetite tends to raise the threshold for stimuli irrelevant to the satisfaction of the appetite involved. The more powerful the appetite the greater the potential danger, and hence the greater the need for isolation. The sexual and nutritional lusts claim first place. Many animals will not eat while under observation, and it is difficult to witness coitus between the male and female of almost any wild species. Savages, who are immodest in many ways according to our prejudices, never perform the sexual act where they can be seen, except, rarely, as a religious rite. Prof. Malinowski writes me: "Eating very definitely tends to be secretive I find from the scanty notices of commensalism among primitive peoples, that eating in company is extremely rare, that eating among strangers is regarded as definitely indecent." Incapacity to deal with potential dangers is still more obvious during illness and sleep. Consequently both animals and men evince a desire for isolation when ill or sleepy that extends from a pure mandatory instinct to a mere tendency. Malinowski[1] generalizes thus: "It is characteristic that sexual activities, sleep and excretion are surrounded by protective taboos and mechanisms of concealment and isolation in every society."

A factor of no mean importance is probably the assumption of a posture in which speedy defence is impossible. This is pre-eminently true of copulation and excretion in a squatting position. So strong is the tendency to seek seclusion for these acts that it persists even in our own civilization, giving rise to curious anomalies in behaviour. My attention has been drawn to one of these by my friend, Dr T. A. Ross, who pointed out that men who urinate shamelessly in public will not only retire for defaecation but will lock the door even when the possibility of interruption is remote. On the other hand, women who assume the same position for both forms of excretion are equally secretive about them, and are loath to exhibit either micturition or defaecation even to their own sex. This is particularly striking in Southern European countries where men urinate shamelessly in public places but never defaecate, whereas women, who will expose their breasts when nursing babies in a most matter-of-fact way, will never indulge publicly in either kind of excretory act. The factor of position is summarized in the American phrase, "To be caught with the pants down," which expresses defencelessness and not shame as it should do in the context of modern social usage.

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[1] Personal communication.

A third factor importing danger into the indulgence of appetites has been suggested to me by Prof. Malinowski. It excites jealousy. Hence to make love or to eat in public is to invite rivals to seize that which is being enjoyed. Our table customs, he thinks, may have as their *raison d'être* the suppression of too expressive delight.

It has frequently been remarked that what constitutes a virtue or a sin varies enormously from one human society to another. This is equally true of shame. To us the naked savage seems immodest, to him one of our restaurants would be horrifying. All the variations can probably be explained by invoking four factors. These are growth of security, the conservatism of customs, the evolution of modesty and the extension of shame through unconscious symbolism. These factors may be briefly treated in the order named.

Primitive man, moving in a universally hostile environment, sought seclusion, we may assume, for all acts which tended to reduce his capacity to defend himself. This is a purely instinctive affair and not necessarily associated with any feeling of shame. In an early society some activities exposed him to danger from his fellows. These had to be indulged secretively. When a stimulus for such an activity produced an impulse towards seclusion at the same time that gregariousness urged him to remain with his mates, we must imagine that a conflict arose and the emotion of shame was born. It became an indication, a betrayal, of an impulse towards action that was biologically solitary and therefore individualistic. Shame has, as one of its components, the feeling of being singled out from the mass. As civilization has advanced personal safety has become more and more secure, and the necessity for isolation in the indulgence of appetites has diminished. There are, therefore, only relics in our society of shame over such practices as eating, sleeping or invalidism. Were potential danger the only factor we ought to have no shame about excretion or licensed sexual intercourse in public and be embarrassed only over definitely anti-social undertakings.

As a matter of fact, of course, the primitive still lives on in us entirely apart from social usage. As I have said, a man shuts himself up to defaecate. Almost everyone is ashamed of falling asleep inadvertently and usually protests that he has been awake all the time, and those of the upper classes object to sleeping at all in anything but a private room. An interesting development is the transference of what was once an individual isolation into the isolation of friends or social groups. Officers and men have separate messes and latrines, one is ashamed to eat with servants, a southerner may prefer to go hungry rather than to eat in the same room with a negro. Among boys, particularly those with rural experience, excretion in the presence of their mates is a sign of comradeship and this reappears in martial life[1]. If we cannot sleep in privacy we prefer our friends or those of our own class as our neighbours. In general it might be said that, emotionally, we have achieved a feeling of security only within our cliques and not with mankind in general.

The second factor, that of convention, accounts for the survival of many occasions for embarrassment. Once any practice is surrounded by taboos and ceremonials these give a social significance that is part of the identity of the group. To modify them is to tamper with the life of the society in question, which the society naturally resents. If one inadvertently does "what isn't done," the feeling of exclusion from the group—the feeling of shame—is coincident with recognition of the mistake. The conservatism of custom unquestionably accounts for the maintenance of a feeling of delicacy about certain topics of conversation, and table manners, for instance. This is well shown in connection with demonstrativeness. As we have seen, indulgence of appetites under primitive conditions is potentially dangerous and they become secretive. The next step is a cloaking of the feelings which these appetities arouse: this becomes conventionalized and then demonstrativeness in general is taboo. The upper classes in all civilized countries are more self-contained than the lower, but the variation from one land to another is enormous. The Englishman says the Frenchman gesticulates like a madman; the Frenchman avers the Englishman is totally without feeling. Either would be ashamed to behave like the other.

Modesty is so important a factor in the evolution of shame that it would be impossible to deal with it at all in such a brief communication as this, were it not that it is safe to assume that you are all familiar with its biological significance as elaborated by Darwin, and its human evolution as delineated by Havelock Ellis. It is present in many animals well below the human level, and is installed as an element in courtship.

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[1] See *All Quiet on the Western Front.*

Biologically, modesty, or shyness in the female, is a temporary reluctance to yield to sexual overtures which serves to lure the ardent male in pursuit and discourage his less energetic rival. In complex human psychology it affects the male as well and, not unnaturally, is soon intertwined with sexual shame. A private, as well as a public taboo, so to speak, is thus laid on sex. This alone would tend to make sex the most important cause for embarrassment in our civilization and, of course, there are other reasons for the elevation of it into a position of supreme notoriety. It would be impossible even to outline them in such a brief communication as this, but I imagine there would be few to deny the claim that shame now tends to centre in and irradiate from sex. It is difficult, for instance, to understand why excretory processes should have retained their claim to concealment during all the ages that separate the savage and the savant, were it not that they necessitate an exposure of the sexual organs.

This brings us to our fourth factor in the variability of shame. Why do some people blush and act as if ashamed when praised in public? This can be taken as an example of individual variation. I have already pointed out that one element of shame is the feeling of being conspicuous. Praise makes one conspicuous. But why should conspicuousness make some people unhappy, when it stimulates others pleasantly? The answer to this question is best given, I believe, in psychoanalytic terms. Modesty and convention demand that the sexual organs—the *pudenda*—should be hidden. On the other hand, display of secondary sexual characters, if not of the organs themselves, is part of the total mating behaviour. Biologically, therefore, we inherit opposed tendencies and, in all of us, there are deep-rooted impulses towards sexual display. In the absolutely normal civilized man or woman exhibitionism gains a slight outlet of a direct kind in connection with licensed sexual activities, but is chiefly satisfied symbolically. In the broadest sense (and therefore, perhaps, most accurately) the peacock who raises and spreads his tail is acting symbolically and the millinery of the human female has a similar function[1]. Even at the animal level there is a transference in the nature of the display from the sexual organs themselves to other parts of the body, to the body as a whole, and to the performances of that body. At the human level the same translation takes place but can go further so as to include the person, the personality, or some special attribute of it. Public recognition of worth can symbolize successful exhibitionism and it does so in the normal individual. But if this symbolism is inadequate as an outlet, there remains, unconsciously, an unsatisfied lust for sexual display, and this can be held in check only by modesty so disproportionate as to be pathological. Any kind of conspicuousness can then affront this modesty and throw the unfortunate into a paralysis of embarrassment[1]. One bit of evidence in favour of this argument is the practically universal habit among men unaccustomed to public speaking of seeking some cover for the pelvic regions. They must stand behind a table, a desk or the back of a chair which will hide the middle of the body. Failure or success in achieving such protection may make all the difference between failure or success in conquering embarrassment. Almost any man can multiply instances of behaviour of this order.

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[1] Complexion in all the white-skinned races is such an important element in beauty that we may perhaps regard the maiden's blush and rosy cheeks, which depend on atonia in the minute vessels in the skin, as secondary sexual characteristics. If so, this localized atonia would have a survival value that would account for the skin of the face not gaining the adaptation to exposure which there is in the skin of the hands.

It may be well in conclusion to recapitulate the argument of this paper. A number of authors have concluded that the behaviour accompanying the emotion of shame represents an instinctive seeking of cover. Blushing is, perhaps, an evidence of the shifting of balance in the involuntary nervous system towards a strengthening of the vagal component at the expense of the sympathetic. This suggests a reaction by the organism towards inactivity and, among the danger responses, this would mean immobility rather than flight or aggression. Immobility among animals turns to hiding in man, and the physiology of blushing and the behaviour of shame are thus identified at primitive levels. Among savages concealment is sought for practices that are potentially dangerous in a hostile environment. These are eating, sleeping, sexual intercourse and excretion. Shame develops in connection with publicity in the indulgence of these four. In modern civilization security has been achieved for the first two and only traces of shame persist in connection with them. Modesty, of biological origin, has over-determined the shame concerning sex and this has been extended to excretion. Finally, symbolism has made possible the appearance of shame in connection with practices that are neither sexual nor potentially dangerous.

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[1] It should be borne in mind that this is a suggested explanation only for anomalous liability to shame. In every society convention decrees a certain limit which is set to public praise: whenever this standard is exceeded, the recipient of the praise is made conspicuous and therefore feels shame.

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ARCHETYPAL PATTERNS IN TRAGIC POETRY.

BY MAUD BODKIN.

I. *Nature of the enquiry and reasons for undertaking it* (pp. 183–186).

II. *Reasons why archetypal patterns should be studied in the experience communicated through poetry* (pp. 187–189).

III. *Value for this purpose of particular passages possessing power of 'incantation'* (pp. 189–191).

IV. *Examination of the Oedipus complex as a pattern determining our imaginative experience of* Hamlet (pp. 191–193).

V. *Further illustration of the theme of conflict between the generations, involving 'splitting' of a type figure, the object of an ambivalent attitude* (pp. 193–195).

VI. *Examination of the form of tragic drama, in relation to Jung's discussion of fantasy figures from the Collective Unconscious* (pp. 195–200).

VII. *Conclusion* (pp. 200–202).

I. NATURE OF THE ENQUIRY AND REASONS FOR UNDERTAKING IT.

IN an article upon "The relation of analytical psychology to poetic art[1]" Dr C. G. Jung has set forth a hypothesis in regard to the psychological significance of poetry. The special emotional significance possessed by certain poems—a significance going beyond any definite meaning conveyed—he attributes to the stirring in the reader's mind, within or beneath his conscious response, of unconscious forces which he terms 'primordial images,' or archetypes. These archetypes he describes as "psychic residua of numberless experiences of the same type," experiences which have happened not to the individual but to his ancestors, and of which the results are inherited in the structure of the brain, *a priori* determinants of individual experience.

It is the aim of the present paper to examine this hypothesis, testing it in regard to examples where we can bring together the recorded experience and reflection of minds approaching the matter from different standpoints. It is hoped that, in this way, something may be done toward enriching the formulated theory of the systematic psychologist through the insight of more intuitive thinkers, while at the same time the intuitive thinker's results may receive somewhat more exact definition.

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[1] *Brit. J. of Med. Psychol.* III (3).

My main illustration I shall take from an essay by Prof. Gilbert Murray[1], where the effect of great poetic drama is described in language somewhat similar to that of Jung. Gilbert Murray has been comparing the tragedies of *Hamlet* and of *Orestes*, noting the curious similarities between them, and how the theme that underlies them seems to have shown an "almost eternal durability." When such themes as stirred the interest of primitive man move us now, he says, "they will tend to do so in ways which we recognize as particularly profound and poetical" (p. 238). Gilbert Murray apologizes for the metaphor of which he cannot keep clear when he says that such stories and situations are "deeply implanted in the memory of the race, stamped as it were upon our physical organism." We say that such themes "are strange to us. Yet there is that within us which leaps at the sight of them, a cry of the blood which tells us we have known them always" (p. 239). And again: "In plays like *Hamlet* or the *Agamemnon* or the *Electra* we have certainly fine and flexible character-study, a varied and well-wrought story, a full command of the technical instruments of the poet and the dramatist; but we have also, I suspect, a strange, unanalysed vibration below the surface, an undercurrent of desires and fears and passions, long slumbering yet eternally familiar, which have for thousands of years lain near the root of our most intimate emotions and been wrought into the fabric of our most magical dreams. How far into past ages this stream may reach back, I dare not even surmise; but it seems as if the power of stirring it or moving with it were one of the last secrets of genius" (pp. 239–240).

We have here an expression, itself somewhat imaginative and poetical, of an experience in presence of poetry which we may submit to closer examination—and this in two ways. We may study the themes that show this persistence within the life of a community or race, and may compare the different forms which they assume; also we may study analytically in different individuals the inner experience of responding to such themes.

The enquiry is plainly of a subtlety and complexity apt to discourage at the outset those who prefer to avoid all questions that cannot be investigated in accordance with a strict technique. There is little possibility here for experiment, since the kind of emotional experience which it is desired to investigate cannot be commanded at will under test conditions. A profound response to great poetic themes can be secured only by living with such themes, dwelling and brooding upon them, choosing those moments when the mind seems spontaneously to open itself to their influence. We must take where we can find it the recorded experience of those who have such acquaintance with poetry.

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[1] "*Hamlet* and *Orestes*," included in *The Classical Tradition in Poetry*, Oxford, 1927.

To the present writer it appears, however, that it is by the study of such deeper experience that psychology at the present time particularly needs enrichment. We might almost say that academic psychologists have been routed by the attack of those medical writers who claim access to the deeper layers of the mind, just because the demand for exact verifiable results has held academic psychologists to the mere outworks or surface of the mind they set out to study. If inexactness of thought or one-sided emphasis has characterized the medical writers, this can be established only by following them along the obscure paths they have opened into the concrete human psyche, and bringing to bear, if possible, a wider ranging interest and a more exact and cautious scrutiny.

The student who seeks to explore the imaginative response of present-day minds to the great themes of poetry may profit by considering the work not only of the medical psychologists, but also of the anthropologists who have attempted to study scientifically the reactions of more primitive minds. In studying the reception by a people of new cultural elements anthropologists have made use of the term 'culture pattern' to designate the pre-existing 'configuration,' or order of arrangement, of tendencies which determines the response of members of the group to the new element. In discussing the value for our 'conceptual explorations' of 'the culture pattern concept,' Goldenweiser[1] has noted its relation to the concept of form and system in the arts and cultural disciplines; and L. L. Bernard, in the same work, has undertaken a classification of different kinds of environment, distinguishing the psycho-social environment, which includes such systems of symbols as are preserved in books, and in which he says "psychic processes reach the highest type of their objectified development." Such stored symbolic content can at any time become effective in activating the corresponding patterns in the minds of members of the group whose collective product and possession the symbols are.

It is within the general field of anthropology or social psychology that I conceive the enquiry to lie which I am here attempting to pursue. I shall use the term 'archetypal pattern' to refer to that within us which, in Gilbert Murray's phrase, leaps in response to the effective presentation in poetry of an ancient theme. The hypothesis to be examined is that in poetry—and here we are to consider in particular tragic poetry—we may identify themes having a particular form or pattern which persists amid variation from age to age, and which corresponds to a pattern or configuration of emotional tendencies in the minds of those who are stirred by the theme.

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[1] *The Social Sciences and their Interrelations*, edited by W. F. Ogburn and A. Goldenweiser, Allen and Unwin, 1928.

In Jung's formulation of the hypothesis, and in the more tentative metaphorical statement of Gilbert Murray, it is asserted that these patterns are "stamped upon the physical organism," "inherited in the structure of the brain"; but of this statement no evidence can be considered here. Jung believes himself to have evidence of the spontaneous production of ancient patterns in the dreams and fantasies of individuals who had no discoverable access to cultural material in which the patterns were embodied. This evidence is, however, hard to evaluate; especially in view of the way in which certain surprising reproductions, in trance states, of old material, have been subsequently traced to forgotten impressions of sense in the lifetime of the individual.

Of more force in the present state of our knowledge is the general argument that where forms are assimilated from the environment upon slight contact only, predisposing factors must be present in mind and brain. Whoever has experienced and reflected upon the attempt to convey an idea, especially an idea of intimate and emotional character, to a mind unprepared to receive it, will have realized that it is not mere contact with an idea's expression that secures its assimilation. Some inner factor must co-operate. When it is lacking, the experienced futility of attempted communication is the most convincing proof that it is, as Mr F. C. Bartlett has said, "not fanciful to hold" that for the capture of objects complete, by the assimilative imagination, there must stir within us "larger systems of feeling, of memory, of ideas, of aspirations[1]." Such systems may be culture patterns confined to a particular group at a certain time, or may characterize a particular individual; but there are others of much wider range. Our question is whether there are some whose "almost eternal durability," in Gilbert Murray's phrase, justifies us in applying to them the term archetypal, and renders them of special interest and importance to the student of psychology and of literature.

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[1] "Types of imagination," *J. of Philosophical Studies*, III (9), 80.

II. REASONS WHY ARCHETYPAL PATTERNS SHOULD BE STUDIED IN THE EXPERIENCE COMMUNICATED THROUGH POETRY.

We come nearer to our particular subject in raising the question: What is the distinctive advantage of having recourse to poetry for the study of these patterns?

Such a theme as that discussed by Gilbert Murray existed as a traditional story in ancient Greece before Aeschylus, and in Northern Europe before Shakespeare handled it. In that form it was already, as A. C. Bradley says of another traditional theme, "an inchoate poem": "such a subject, as it exists in the general imagination, has some aesthetic value before the poet touches it"; "it is already in some degree organized and formed[1]." Enriched by the poet's touch, the traditional story lives on in our imagination, a memory with aesthetic value, but fading into formlessness, when, as perhaps now in the mind of the reader, the reference to Orestes or to Hamlet excites only a faint recollection of what was once a vivid poetic experience. When, therefore, we desire to examine psychologically the emotional pattern corresponding to a poetic theme, we may sometimes avail ourselves of references to the mere tales recalled in outline, but for closer examination there is need that the actual poetic experience be recovered, since it is in the imaginative experience actually communicated by great poetry that we shall find our fullest opportunity of studying the patterns that we seek—and this from the very nature of poetic experience.

In the writings of Prof. Spearman, which have had so much influence in the determining of psychological method, there are references to imagination—one particularly to imagination as exercised in poetry[2]—in which he asserts that imagination in its intellectual aspect does not differ essentially from any other logical process in which new content may also be said to be generated, as when, from a given term, say 'good,' and a knowledge of the nature of verbal opposition, we pass to the term, 'bad.' Such a treatment of imagination illustrates the kind of abstraction that makes psychology, to some thinkers, appear so unreal and empty a study. A student who is interested in imaginative activity as exercised in poetry cannot accept the view that its intellectual aspect can be separated from its emotional nature and covered by any such logica formula as Spearman proposes.

Of the three laws of cognition which Prof. Spearman formulates it would seem to be the first which most nearly concerns the poetic imagination. This law is stated: "Any lived experience tends to evoke immediately a knowing of its characters and experiences." A note adds: "The word 'immediately' means here the absence of any mediating process[1]." It is perhaps within this mediating process, denied by Spearman, that we may find a distinctive place for imagination as exercised in poetry.

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[1] *Oxford Lectures on Poetry*, pp. 11–12, Macmillan, 1909.

[2] *The Nature of Intelligence and the Principles of Cognition*, pp. 334–336, Macmillan, 1927.

When psychologists have raised the question: How does lived experience come to awareness? they have usually been content to assert that it happens through introspection, and to leave to philosophers any further investigation of the question. It is here that difficulty has arisen between the academic and the medical psychologists; since the latter believe themselves to have discovered large ranges of lived experience, of conative character, of which introspection can give no account—a discovery that seems surprising to those who believe that in introspection we have direct access to the nature of our desires.

Prof. S. Alexander[2], examining the question as a philosopher, concludes that lived experience, which is of conative character—as distinct from sensations and images, the objects of the mind—can only be 'enjoyed'; it cannot be contemplated. Introspection, he says, is 'enjoyment' lived through, together with "a whole apparatus of elaborated speech" (I, 18), which causes the elements of the experience enjoyed to stand out in "subtly dissected form" (p. 19). "It is small wonder," he adds, "that we should regard our introspection as turning our minds into objects, seeing how largely the language which expresses our mental state has been elaborated in pursuit of practical interests and in contact with physical objects."

If this view is accepted, we see that the mediating process necessary before lived experience can come to awareness is the linking of such experience with actions and objects that affect the senses and can be contemplated, and with words that recall these objects in all their variety of human perspective. It is in the process of fantasy that the contemplated characters of things are broken from their historical setting and made available to express the needs and impulses of the experiencing mind. The recent study of dreams appears to have made it certain that the bewildering sequence of the images thrown up by the sleeping mind is due to processes of interaction between emotional dispositions lacking the customary control. In individual waking fantasy, and in myth and legend, we have other sequences of images which emotional patterns determine, and which seem to us strange as dreams, when, repeating them in the words used also for the results of logical reflection, we are led to contrast these incompatible renderings of experience.

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[1] *The Nature of Intelligence and the Principles of Cognition*, p. 48.

[2] *Space, Time, and Deity*, Macmillan, 1920.

When a great poet uses the stories that have taken shape in the fantasy of the community, it is not his individual sensibility alone that he objectifies. Responding with unusual sensitiveness to the words and images which already express the emotional experience of the community, the poet arranges these so as to utilize to the full their evocative power. Thus he attains for himself vision and possession of the experience engendered between his own soul and the life around him, and communicates that experience, at once individual and collective, to others, so far as they can respond adequately to the words and images he uses.

We see, then, why, if we wish to contemplate the emotional patterns hidden in our individual lives, we may study them in the mirror of our spontaneous actions, so far as we can recall them, or in dreams and in the flow of waking fantasy; but if we would contemplate the archetypal patterns that we have in common with men of past generations, we do well to study them in the experience communicated by the great poetry that has continued to stir emotional response from age to age. In studying such poetry here, we are not asking what was in the mind of Aeschylus or of Shakespeare when he fashioned the figure of Orestes or of Hamlet, nor do we ask how these figures affected a Greek or an Elizabethan audience. The question is between the writer and the reader of this article: what do the figures of Orestes and Hamlet stand for in the experience communicated to us, as we see, read, or vividly recall the Greek or Shakespearian tragedy?

III. VALUE FOR THIS PURPOSE OF PARTICULAR PASSAGES POSSESSING POWER OF 'INCANTATION.'

A preliminary difficulty, already touched on, must be considered in more detail. How can we secure that the experience communicated by a great play shall be present to us with such completeness and intensity that we can make adequate study of it?

A parallel question has been discussed by Percy Lubbock[1] in regard to the study of the form of a novel. Critical perception, he says, is of no use to us if we cannot retain the image of a book, and the book reaches us as a passage of experience never present in its completeness. The task of the reader, before he can criticize, is to refashion the novel out of the march of experience as it passed. The procession "must be marshalled and concentrated somewhere" (p. 15).

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[1] *The Craft of Fiction*, The Travellers' Library, 1926.

In watching a play adequately interpreted upon the stage, we find, perhaps more readily than in the silent reading of a novel, that the procession of experience is marshalled and concentrated at certain points; so that, recalling the images of these, we can look back upon the whole play as a living unity. The powerful emotional impression thus attained may persist while the play is read and recurred to again and again, and the individual impression clarified by comparison with the reflective results of critics and scholars. Central passages, while the play is thus lived with, grow ever richer in meaning, becoming intertwined with the emotional experience of one's own life.

Some such experience of *Hamlet* I must presume in the reader, since I cannot afford space to recall the play at all fully. I will venture, however, to refer to that passage in which for me the significance of the whole play seems most concentrated.

From the experience of seeing *Hamlet* performed some thirty years ago, there has remained with me the memory of the strange exaltation and wonder of beauty that attended the words of the dying Hamlet to Horatio:

> If thou didst ever hold me in thy heart,
> Absent thee from felicity awhile,
> And in this harsh world draw thy breath in pain,
> To tell my story.

This is one of the passages chosen by Matthew Arnold[1] as 'touchstones' for poetry—passages possessing both in style and substance supreme poetic quality, the Aristotelian high truth and seriousness, beyond that of history or ordinary speech. I would suggest that this 'high truth' of which Matthew Arnold speaks—like that character attributed by Lascelles Abercrombie to great poetry, "a confluence of all kinds of life into a single flame of consciousness"—belongs to the lines not as isolated, but as grown familiar in their setting—the unified experience of the play converging upon them and the incantation of their music carrying them ever deeper into the secret places of the mind that loves them.

One may make some attempt to analyse that 'incantation[2],' or enchantment, in which rhythm and sound of words evidently play a part. It seems to me that the enchantment of the line, "Absent thee from felicity awhile," is heightened by the later echoing of its sounds in the lines spoken by Horatio:

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[1] "The study of poetry," *Essays in Criticism*, 2nd series, 1889.

[2] Cf. Lascelles Abercrombie, *The Idea of Great Poetry*, 1925, p. 19.

> Good night, sweet prince,
> And flights of angels sing thee to thy rest!

Against this music of heaven we feel more poignantly the contrast of the words "in this harsh world draw thy breath in pain" that move, labouring, toward their goal in the words "to tell my story." Through their power of incantation these words, as they fall in their place, seem to gather up all the significance of that struggle of a powerful impulse to action against an obscure barrier, all the impotent anger, and perplexity, and longing for justification and release, that make up the story of Hamlet, as Shakespeare tells it, and make also of Hamlet, and of these lines, a symbol for whatever such struggle and longing has tortured the mind that is responding to Hamlet's words.

IV. EXAMINATION OF THE OEDIPUS COMPLEX AS A PATTERN DETERMINING OUR IMAGINATIVE EXPERIENCE OF *HAMLET*.

Before attempting to compare, with reference to underlying emotional pattern, *Hamlet* and the plays concerned with Orestes, we may consider briefly the study made of *Hamlet* by Dr Ernest Jones[1].

Dr Jones, in exploring the nature of Hamlet's conflict, has to some extent followed the same line of enquiry that I am pursuing here. For what is it that the critic is actually doing when he traces the motives of a character in a play?

In projecting the figure of a man of a certain disposition and analysing the forces behind his behaviour, the critic is inevitably using the emotional experience which he himself undergoes in living through the play. Having experienced, as communicated by the speeches of Hamlet, a certain psychological movement in which a strong impulse to action is aroused, and again and again sinks back into apathy and despair—a movement which, while imaginatively experiencing it, the reader imputes to the fictitious speaker—afterwards, in reviewing the total impression so received, with analysis and synthesis of its successive movements, the critic discerns, so far as his thought does not deceive him, within the fictitious personality of Hamlet, the reflected pattern of the emotional forces that have operated within his own imaginative activity.

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[1] "A psycho-analytic study of *Hamlet*." Included in ***Essays in Applied Psycho-analysis***, 1923.

To Dr Jones the conflict of Hamlet appears an example of the working of the Oedipus complex. Hamlet cannot whole-heartedly will the slaying of his uncle, because "his uncle incorporates the deepest and most buried part of his own personality." The repressed desire for the death of his father and the sexual enjoyment of his mother, persisting unconsciously from infancy, has produced an unwitting identification by Hamlet of himself and his guilty uncle, so that only at the point of death, "when he has made the final sacrifice...is he free...to slay his other self" (p. 57).

This psychological hypothesis, contributed by Freud and elaborated by Dr Jones, has been welcomed by certain literary critics. Herbert Read, referring to the view of J. M. Robertson, that *Hamlet* is "not finally an intelligible drama as it stands"—that Shakespeare could not make a psychologically consistent play out of his barbaric plot and supersubtle hero—urges that however baffling to critics the play may have proved, nevertheless in experiencing it we are aware of a personal intensity of expression, a *consistent* intensity, which gives the play a unity which the older academic critics lacked means to explore[1]. Dr Jones' hypothesis, he considers, does serve to explain this acceptance by our feeling of any difficulties and incoherence which our thought may find. Using the terms I have suggested, we might say that the hypothesis of the Oedipus complex—*i.e.* of a persistent unconscious wish, hostile to the father and dishonouring to the mother, in conflict with the sentiment of filial love and loyalty—offers to our thought an emotional pattern which does correspond to the play of feeling stimulated during a full imaginative participation in the drama. Prof. Bradley has spoken of *Hamlet* as deserving the title of "tragedy of moral idealism[2]," because of the intensity, both of idealizing love and of horror at betrayal of love, that we feel in Hamlet's speeches. He dwells upon the shock to such a moral sensibility as Hamlet's of witnessing the faithlessness of his mother and uncle; yet there seems a discrepancy between the horror and disgust that a sensitive mind might naturally feel at such faithlessness in others and the overwhelming disgust that Hamlet feels, at himself, his whole world and his attempted action, unless we realize that he feels the treachery of his mother and uncle echo within himself and within the sentiment of loyal love to his father that is his strongest conscious motive. If, in reviewing the experience communicated by the play, we conceive a loyal love undermined, as it were, by a bewildered sense of treachery within as well as without, we must, I think, agree that the Freudian hypothesis does throw some light upon that intimate immediate experience which is the final touchstone of critical theory.

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[1] Herbert Read, *Reason and Romanticism*, p. 101, Faber and Gwyer.

[2] *Shakespearian Tragedy*, p. 113, Macmillan, 1912.

V. FURTHER ILLUSTRATION OF THE THEME OF CONFLICT BETWEEN THE GENERATIONS, INVOLVING 'SPLITTING' OF A TYPE FIGURE, THE OBJECT OF AN AMBIVALENT ATTITUDE.

Perhaps the most important contribution that has been made by the Freudian theory of dream interpretation to the understanding of the emotional symbolism of poetic themes is that concerned with the 'splitting' of type figures. In comparing the Hamlet story with the story of Oedipus, Dr Jones asserts that both are variants of the same motif, but in one the father figure remains single, while in the other it is 'split into two'—the father loved and revered, and the hated tyrannical usurper.

This assertion involves two elements of hypothesis:

(1) The fundamental assumption—implied also in the statements of Jung and Gilbert Murray, with which this discussion opened—that these ancient stories owe their persistence, as traditional material of art, to their power of expressing or symbolizing, and so relieving, typical human emotions.

(2) That the emotion relieved is in this case the two-sided—ambivalent—attitude of the son toward the father. Let us examine this latter hypothesis more closely.

It appears to be characteristic of the relation between father and son that the father should excite in the son both feelings of admiration, love and loyalty, and also impulses of anger, jealousy, and self-assertion. The more the son learns to 'idolize' his father, developing what Shand has called the 'conscience of the sentiment,' so that any muttering of jealousy or hostile criticism is suppressed as disloyal, the more acute will become the tension of the inner attitude. It is such an attitude that can find relief in imaginative activity wherein both the love and the repressed hostility have play. In the story of Oedipus, according to the Freudian hypothesis, a repressed persistent impulse to supplant the father and enjoy the mother finds expression in the first part of the action; then in the latter part, in the hero's remorse and suffering, appears the expression of the sentiment of respect and loyalty. In the

Hamlet legend—as it appears, *e.g.* in the Amleth Saga—combined fear and hostile self-assertion against the father find expression through all the incidents of simulated stupidity, and secret bitter word-play, and at last in the achievement of the plotted slaying of the usurper; while at the same time the sentiment of love and loyalty is triumphantly expressed in that same act of filial vengeance. It is Shakespeare only who appears to have brought into the rendering of the ancient story the subtle factor of the division and paralysis of the will of the hero, by the intuitive apprehension that the impulse that drove his uncle against his father, was one with that present in himself.

The story of Orestes may be considered as another example of the imaginative expression of the ambivalent attitude of child toward parent. In this story, as presented by the three great Attic tragedians, there is a wealth of material, if the limits of our space allowed of its examination, for illustrating the manner in which the mysterious inner forces of emotion may, through the symbolic shapes created by imagination, be made palpable to sense. But we must be content here to consider briefly only the outline of the story.

Considered as a variant of the Hamlet theme, its distinctive note is that the usurper upon whom the son's fierce self-assertion and craving for vengeance strike is not alone the male kinsman, but also the queen-mother, who has betrayed, and with her own hands murdered the father. Therefore the moment of triumphant self-assertion, when the son has proved his manhood, and vindicated his loyalty upon his father's enemies, is also the moment when there awakens, the palpable, pursuing horror of the outraged filial relation—since this enemy was also a parent, the mother of the slayer.

The conflict presented in the Orestes dramas is plainly concerned not with sex, but with combined love and hate of either son or daughter converging upon a parent figure which may be either father or mother. It is the enduring conflict between the generations which continues to find expression in the story, when more temporary questions—such as that between patriarchy and mother-right, which may have been present in Athenian minds—are no longer urgent. That this theme of conflict between the generations had great significance within the sensibility that found expression in Shakespeare's plays, is evident from the tragedy of *King Lear*.

In this drama the emotional conflict between the generations is communicated from the standpoint of the old man, the father who encounters in separate embodiment in his natural successors, the extremes of bestial self-seeking, and of filial devotion. Bradley has noted how the play illustrates "the tendency of imagination to analyse and subtract, to decompose human nature into its constituent factors[1]." This mode of thought, he suggests[2], is responsible for "the incessant references to the lower animals" which occur throughout the play. Thus Goneril is likened to a kite, a serpent, a boar, a dog, a wolf, a tiger. This analysing work of the imagination, separating the bestial and the angelic in human nature and giving them distinct embodiment, in the wicked daughters and Cordelia (and again in Edmund and Edgar, the cruel and the loyal sons of Gloucester), presents another instance of what we have already observed in the 'splitting' of the father figure. The splitting in this play is from the point of view of the parent; as, in the Orestes or Hamlet story, it is from the point of view of the child. As, to the feeling of the child, the parent may be both loved protector and unjustly obstructing tyrant, and these two aspects find their emotional symbolism in separate figures in the play; so, to the feeling of the parent, the child may be both loving support of age and ruthless usurper and rival, and these two aspects find expression in separate figures, such as the tender and the wicked daughters of Lear.

VI. EXAMINATION OF THE FORM OF TRAGIC DRAMA, IN RELATION TO JUNG'S DISCUSSION OF FANTASY FIGURES FROM THE COLLECTIVE UNCONSCIOUS.

We have considered, so far, the emotional pattern corresponding to a particular theme—the conflict between the generations—which, though a recurring one, is by no means co-extensive with the realm of tragic drama. Can we identify an archetypal pattern corresponding to tragedy itself—its universal idea or form?

Gilbert Murray, taking the "essential tragic idea" to be that of "climax followed by decline, or pride by judgment," and attempting a closer analysis of this sequence, maintains that what is 'really characteristic' of the tragic conflict is "an element of mystery derived ultimately from the ancient religious concepts of *katharsis* and atonement[3]." The death or fall of the tragic hero has in some sense the character of a purifying or atoning sacrifice.

In considering this conception we may first remedy an inadequacy that the reader has probably noticed in the previous discussion of the dramas of Hamlet and Lear, Orestes and Oedipus. We have so far ignored the royal status of the father and son concerned in the tragic conflict. Yet the kingly status has great significance for the feeling expressed in the play.

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[1] *Shakespearian Tragedy*, p. 264, Macmillan, 1912.

[2] *Ibid.* p. 266.

[3] *Op. cit.* "Drama," p. 66.

Consider, for instance, the tragedy of *King Lear*. "The master movement of the play," says Granville Barker, is Lear's passing "from personal grievance to the taking upon him, as great natures may, of the imagined burden of the whole world's sorrow[1]." It is Lear's royal status that helps to make this movement possible. King Lear is at once "a poor, infirm, weak and despised old man"—a father broken to tears and madness by his daughters' cruelty, and also in his sufferings a superhuman figure—one who can bid the "all shaking thunder strike flat the thick rotundity of the world" in vengeance for his wrongs. It is in part the associations with which history, and pre-history, has invested the name and image of king that make it possible for us, under the spell of Shakespeare's verse, to accept the figure of Lear as in this way exalted in his agony beyond human stature. His madness, his pitiful humanity, appear, according to that comment which Shakespeare puts into the mouth of an attendant, on behalf of all onlookers, "a sight...past speaking in a king." The word 'king' is here, through its position in the play, loaded with a significance for the sources of which we must go far back in the story of the race and of the individual.

It is probably because, to the mind of the young child, the father appears of unlimited power that in the life-history of the individual imagination the figures of father and king tend to coalesce. Legends and fairy stories that reflect the feelings of more primitive people toward their king are interpreted by the child in the light of his own earlier feeling toward his father. In the case both of the child and of the primitive individual the same process seems to take place—an emerging of the consciousness of self from out a matrix of less differentiated awareness, which may be called collective or group-consciousness. The figures of both father and king tend to retain, within those deeper levels of the mind to which poetry may penetrate, something of the *mana* that invested the first representative of a power akin to, but vastly beyond, that of the individual emerging into self-consciousness.

It is this supernatural aspect which the father-king of tragic drama has for the kindled imagination that is of importance when we try to understand the element of religious mystery which is characteristic of tragedy—plainly in the past, and, as Gilbert Murray holds, in some subtler fashion still in our experience to-day.

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[1] H. Granville Barker, *Prefaces to Shakespeare*, 1927, 1st series, p. 171.

Upon this character of tragedy and the tragic hero it is possible, I think, to gain a certain fresh light from a consideration of the conclusions at which Dr Jung has arrived through his study of fantasy figures appearing in personal analysis.

In *The Psychology of the Unconscious*, in the chapter entitled "The Sacrifice," he examines the symbol of the dying hero as it appears in individual fantasy, representing, according to his interpretation, an inflated infantile personality—a childish self that must be sacrificed, if the libido is to move forward into active life—and in a later work he discusses, under the title of the "Mana Personality[1]," a hero figure which he finds appearing with a richer content at late stages of analysis.

It is especially at times when barriers of personal repression are removed and images of 'cosmic' character are arising freely, that the fantasy figure may appear of some great prophet or hero who tends to assume control of the personality[2]. If the conscious or practical personality is poorly developed there is the greater likelihood that it will be overwhelmed when such powerful images rise from the unconscious.

As a literary example of such a case, parallel to actual ones within his experience, Jung accepts H. G. Wells' story of Preemby[3], "a small, irrelevant, fledgling of a personality," to whom is presented in dream and fantasy the figure of Sargon, King of Kings, in such compelling fashion that he is led to identify himself with it. Jung observes that here "the author depicts a really classical type of compensation[4]," and we may compare it with the type of compensation which occurs in connection with what we have already considered as the ambivalent attitude toward a parent.

If, within the conscious life, in relation to a parent, only reactions of admiration and affection are recognized, while other reactions, of hostile character, excited within the brain, are repressed, it is these latter that tend to present in dreams a parent figure as object of violence or contempt. Similarly, if within the conscious life the personal self comes to be known only as "an onlooker, an ineffective speechless man," utterly insignificant; while yet, within the life that animates that particular brain, strong reactions are excited of sympathetic exultation and delight at imaginative representations of human achievement—as the little Preemby of Wells thrilled at "the mystery of Atlantis and of the measurements of the Pyramids"—then there may arise, as compensatory to the belittled self, the figure of a hero-self, or *mana* personality, fashioned, as it were, from the stuff of these imaginative reactions; just as the figure of the hated father was fashioned from the energy of the repressed hostility, in compensation for the over-idealizing love.

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[1] *Two Essays on Analytical Psychology*, 1928, translated by H. G. and C. F. Baynes, Second essay, ch. IV.

[2] An interesting autobiographical account of this condition may be found in the writings of E. Maitland (*The Story of Anna Kingsford and Edward Maitland and of the New Gospel of Interpretation*, 1st edition, 1893). See especially the passage where he describes the first arrival of the authoritative 'presence,' and the voice distinctly heard: "at last I have found a man through whom I can speak."

[3] *Christina Alberta's Father*, 1925.

[4] *Op. cit.* p. 193.

The Preemby of Wells' story is saved from his delusion, after many sufferings, by learning to think of his vision as of the spirit of man and its achievements—an inheritance belonging no more to him personally than to every other man. In the same manner every individual in whose fantasy such mighty ghosts arise, with their superhuman claims and relationships, must learn to distinguish such claims from those of the personal self; while yet the personal self may be enriched through the conscious experience of its relation to the great forces which such figures represent.

In this way, according to the view of Jung—by interpreting and giving conscious direction to the 'pure nature-process[1]' of fantasy in which compensatory images arise—such fantasy can become instrumental to the purging of the individual will and its reconciliation with itself. Is it in some such fashion as this that tragic drama, deeply experienced, now or in the past, exercises the function of purgation or atonement in relation to the passions of the spectator?

We cannot attempt here anything like a complete answer to this question. We may, however, with the question in view, examine a little further the nature of the emotional experience of tragic art, still using the examples of *Hamlet* and of *King Lear*.

Prof. Bradley, in examining the experience of tragedy, cites these dramas as examples of tragedies at whose close we feel pain mingled with something like exultation. There is present, he declares, "a glory in the greatness of the soul," and awareness of an "ultimate power" which is "no mere fate," but spiritual, and to which the hero "was never so near...as in the moment when it required his life[2]."

I quote these statements of Bradley, not of course as universally acceptable, but as the attempt of one eminent critic to render his own deeply pondered experience of tragic drama. The experience is rendered in terms rather of philosophy or religion than of psychology. Can we translate it into any more psychological terms? What is this spiritual power, akin to the characters, and, in some sense, a whole of which they are "parts, expressions, products[1]"? I would propose (following the view set forth by F. M. Cornford) the psychological hypothesis that this power is the common nature lived and immediately experienced by the members of a group or community—"the collective emotion and activity of the group[2]." This common nature can, in Alexander's phrase, be enjoyed, but never directly contemplated. As unfathomable to introspection, it is termed by Jung the Collective Unconscious—the life-energy that in its spontaneous movement toward expression generates alike the hero figures of myth and legend and the similar figures that, appearing in individual fantasy, may overwhelm the personal consciousness.

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[1] *Op. cit.* p. 258.

[2] *Oxford Lectures on Poetry*, p. 84.

According to Bradley, the tragic exultation that we feel at the close of *Hamlet* is connected with our sense that the spiritual power of which Hamlet is in some manner the expression or product, is receiving him to itself. It would be this same sense that, as Bradley observes[3], demands, and is satisfied by, the words of Horatio, introducing, against Shakespeare's custom, the reference to another life: "flights of angels sing thee to thy rest." If, as I suggest, the spiritual power, which the philosopher analysing his poetic experience is constrained to represent, be conceived psychologically as the awakened sense of our common nature in its active emotional phase, then our exultation in the death of Hamlet is related in direct line of descent to the religious exultation felt by the primitive group that made sacrifice of the divine king or sacred animal, the representative of the tribal life, and, by the communion of its shed blood, felt that life strengthened and renewed. Hamlet, though he dies, is immortal, because he is the representative and creature of the immortal life of the race. He lives, as he desired to live, in the story with which he charged Horatio—and us who, having participated in that story, descend from the poetic ecstasy to draw breath again in the harsh world of our straitened separate personalities.

The insight of Nietzsche, who knew at once the intoxication of the artist and the analytic urge of the philosopher, discerned the essential nature of tragedy as a vision generated by a dance[4]. The dance of rhythmical speech, like the dance of the ancient chorus, excites the

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[1] *Shakespearian Tragedy*, p. 37.

[2] F. M. Cornford, *From Religion to Philosophy*, p. 87, Arnold, 1912.

[3] *Op. cit.* p. 147.

[4] See *The Birth of Tragedy*, Section 8.

Dionysian ecstasy wherein arises, serene and clear, the Apollonian vision of the imaged meanings the dancing words convey.

The painful images within the vision are at once intimately known and felt, and also 'distanced' like the objects in a far stretching landscape, 'estranged by beauty.' So far as the memory material used by the imaginative activity comes from personal experience, it has undergone "separation...from the concrete personality of the experiencer" and "extrusion of its personal aspects[1]"; but experience is also used which has never been connected with the personal self—as when, in *King Lear*, Shakespeare causes the actor to "impersonate Lear and the storm together[2]," and in the storm "it is the powers of the tormented soul that we hear and see[3]." Here, dramatist, actor and spectator are using experience which was never personal, but shaped through previous apprehension of physical storms into which was imaginatively projected that same impersonal emotional energy from which the daemonic figure of the hero is now fashioned.

To the impersonal, 'distanced,' vision corresponds, in Schopenhauer's phrase, 'a Will-free subject,' one indifferent to the aims and fears of the ego—not held to its private perspective[4].

This felt release, and Dionysian union with a larger whole, would seem to constitute that element of religious mystery—of purgation and atonement—traditionally connected with the idea of tragedy.

VII. CONCLUSION.

If now, summing up our results, we recur to the question: what determining emotional pattern corresponds to the form of tragedy? we may answer first, in accordance with our earlier discussion, that the pattern consists of emotional tendencies of opposite character which are liable to be excited by the same object or situation, and, thus conflicting, produce an inner tension that seeks relief in the activity either of fantasy, or of poetic imagination, either originally or receptively creative. The nature of the opposed tendencies that find relief through diverse renderings of the essential tragic theme, the death or fall of a hero, it is not easy to describe, at once with conciseness and adequacy. But we may attempt this through the concept of an ambivalent attitude toward the self.

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[1] E. Bullough, "Distance as an aesthetic principle," *Brit. J. of Psychol.* v (2), 116.

[2] Granville Barker, *Prefaces to Shakespeare*, p. 142.

[3] A. C. Bradley, *Shakespearian Tragedy*, p. 270.

[4] This character of the aesthetic experience is vividly expressed, in imaginative form, in the lines of De la Mare:

"When music sounds, all that I was I am
Ere to this haunt of brooding dust I came."

Here we have the felt contrast between the subject of the aesthetic experience—"all that I was I am"—and the self that is bounded in space and time by the bodily organism—"this haunt of brooding dust."

In the gradual fashioning and transforming, through the experience of life, of an idea of the self, every individual must in some degree experience the contrast between a personal self—a limited ego, one among many—and a self that is free to range imaginatively through all human achievement. In infancy and in the later years of those who remain childish, a comparatively feeble imaginative activity together with an undisciplined instinct of self-assertion may present a fantasy self—the image of an infantile personality—in conflict with the chastened image which social contacts, arousing the instinct of submission, tend to enforce. In the more mature mind that has soberly taken the measure of the personal self as revealed in practical life, there remains the contrast between this and the self revealed in imaginative thought—well nigh limitless in sympathy and aspiration.

Within what McDougall calls the self-regarding sentiment these contrasting images, and the impulses that sustain and respond to them, may bring about persistent tension. The experience of tragic drama both gives in the figure of the hero an objective form to the self of imaginative aspiration, or to the power-craving, and also, through the hero's death, satisfies the counter movement of feeling toward the surrender of personal claims and the merging of the ego within a greater power—the 'community consciousness.'

Thus the archetypal pattern corresponding to tragedy may be said to be a certain organization of the tendencies of self-assertion and submission. The self which is asserted is magnified by that same collective force to which finally submission is made; and from the tension of the two impulses and their reaction upon each other, under the conditions of poetic exaltation, the distinctive tragic attitude and emotion appears to arise.

The theme of the conflict between the generations—considered earlier, in relation to Hamlet and Orestes, as corresponding to an ambivalent atittude toward a parent figure—is plainly related to this more general theme and pattern; since, as we saw, the same underlying emotional associations cling to the images of father and of king. In experiencing imaginatively the conflict of the generations, the spectator is identified with the hero both as son, in his felt solidarity with the father and revolt against him, and again, when, making reparation for the 'injustice' against his predecessor, he gives place to a successor, and is reunited with that whole of life whence he emerged[1].

One or two points in regard to the argument may be briefly reviewed.

The question is sometimes asked whether the creative activity of the poet and the imaginative response of the reader are sufficiently alike, psychologically, to be considered together. In this paper I have been concerned primarily with imaginative response, and have not attempted to consider the distinctive activity of original composition. In so far, however, as the poet's work, *e.g.* a play of Shakespeare, does reveal his imaginative response to material communicated to him by others and by him to us, I have of course been concerned with the poet's experience.

The concept of racial experience enters the present essay in two ways: (1) all those systems or tendencies which appear to be inherited in the constitution of mind and brain may be said to be due to racial experience in the past. It is not necessary for our purpose to determine exactly the method of this 'biological inheritance' from our ancestors. Of more importance for our purpose is the question concerning (2) the racial experience which we may 'enjoy' in responding to that 'social inheritance' of meanings stored in language which also comes to us from our ancestors, and wakens into activity the potentialities of our inherited nature. In such racial or collective experience as we have discussed in relation to tragic poetry, so far as there is reference to an experiencer, this seems to be not an individual, but rather that larger whole from which what we know as the individual, or personal, self has been differentiated, and which remains with us as the sense, either latent or active, of a greater power.

In the present paper it is maintained that racial experience in this sense is an important factor in the total experience of tragic drama, at the present day, as in the ritual dance from which drama arose. In regard to this question further examination of the imaginative experience can alone be decisive.

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[1] Cf. the mystic saying of Anaximander, concerning the cycle of birth and death, wherein things "give reparation to one another and pay the penalty of their injustice," and the discussion of it by F. M. Cornford, *op. cit.* See especially pp. 8, 147, 176.

(*Manuscript received* 10 *October*, 1929.)

To the EDITOR of the *British Journal of Psychology*.

Would Prof. Pear give us his reasons for preferring to use 'euphasia[1]' rather than 'verbal skill'?

I have to use the concept and have no difficulty in conveying my meaning by the words 'verbal skill': sometimes if my audience has little verbal skill, I define it by the phrase 'skill in the use of words.'

Surely it is better to convey one's meaning by the use of the words 'verbal skill' than to display one's verbal skill by the use of the word 'euphasia'?

Let us inspect the negative. Do not such substitutes as 'kakophasic' for 'verbally unskilled' imply a pathological delight in strange words to the detriment of the normal use of language, which is to represent objects to someone else as easily as possible?

The mere use of names derived from a dead language does not add to knowledge but often takes away the little some people might acquire by fostering the delusion that speech habits are in themselves knowledge of things. Certainly scientists often have to find new words for new concepts, and rightly reject popular words with many meanings already attached. But we have here a concept which is not confused by existing words. Although the words 'verbal' and 'skill' are in general use, their combination is sufficiently rare to be used according to one definition by scientists. The combination eminently fits the concept and there seems to be no sound reason for multiplying technical terms needlessly in this case.

Moreover, the proposed word 'euphasia' has been formed on analogy with 'aphasia'; now 'aphasia' means "loss of speech as a result of cerebral affection": it is not, therefore, a psychological but a physiological term. Similarly also dysphasia. Euphasia according to the analogy would be equivalent to "the physiological speech mechanism is O.K." Now it is the psychological speech mechanism that we are concerned with. A man may have perfect vocal organs and all the rest of the physiological speech apparatus, but he may stammer from fear, be lost for words out of ignorance, or say the wrong thing for a host of reasons that have nothing to do with physiology as such.

Finally, the word 'verbal' covers both the case of those who only speak and those who can only write their verbal descriptions, criticisms and directions of skilled movements. The term 'euphasia' would not cover the man who can *write* the best text-book of a sport. The term 'verbal skill' conveniently covers both vocal and scriptal forms of this ability.

On the grounds that it is a physiological term, an unnecessary term, and an inadequate term, I hope that we shall hear no more of 'euphasia' (except from physiologists) and yet much more of Prof. Pear's interesting researches into the varieties of verbal skill.

H. D. JENNINGS WHITE.

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[1] This *Journal*, xx (4), 371.

PUBLICATIONS RECENTLY RECEIVED

Systematic Psychology: Prolegomena. By E. B. TITCHENER. New York: The Macmillan Company. 1929. Pp. xi + 278. 10*s.* 6*d.* net.

Everybody who knew Prof. E. B. Titchener agreed that his death was an exceedingly great loss to psychology. To many students who had not the pleasure of his personal friendship but who have wrestled with growing hopelessness with the immense detail of his Experimental Psychology, or who have tried in the laboratory to push through his meticulous introspectionism, it must have seemed that many of the things said then were somewhat exaggerated. If, however, they read this book seriously and carefully they will be inclined to agree that all of the tributes fall short of the real truth. Whether Titchener's attempt to characterise science in general and psychology in particular as a 'point of view' carries conviction is relatively unimportant. The book is delightfully written, a model of condensation, alive with learning and devotion. Here is to be seen a man of outstanding ability honestly trying to work out for himself what are the character and purpose of science in its varied expressions and scorning labour in the attempt. To Titchener science sets out to furnish the simplest possible description of the existential world and psychology pursues this aim in regard to sensorial material. Obviously the view rests upon assumption with regard to description, existence and sensation which many people will find it impossible to make or to accept. It also depicts the scientist as much more of a logician than a study of actual scientific procedure would seem to warrant. But in the course of his examination of the view adopted Titchener gives accounts of the work of Wundt, Avenarius, Brentano, Lipps, Meinong and Husserl which, in many respects, are far the best that have ever appeared in English. And not of these writers alone, but of a host of others whom he had studied with rare persistence and an honest attempt at understanding. It is refreshing to come across a book of this thoroughgoing seriousness and candour at a time full to overflowing of facile and superficial studies thrown up in the name of psychology.

The Foundations of Experimental Psychology. Edited by CARL MURCHISON. International University Series. Clark University Press (London: Humphrey Milford). 1929. Pp. x + 907. 27*s.* net.

No psychological library can afford to be without this book, and all serious students of psychology will gain much by studying it carefully. Criticism is easy enough and is, in a way, asked for in the brief Preface where the editor declares that problems not dealt with are ruled out as "promising little return from experimental effort." There is nothing specifically on attention (or, if people prefer the word, on 'attitudes'), perception, imagery and the higher mental processes. The whole conational side of psychology tends to receive only occasional sidelong glances; and perhaps some general indication could have been given of several of the promising applications of psychology to practical problems. Yet clearly not everything could have been included, and though on the whole the choice of topics seems to have been dictated by behaviouristic leanings, it is better to learn from what is included than to cavil at what is not. Naturally also, since each topic is dealt with by a different authority, there is some discrepancy of view and a great amount of variation in the value of the various contributions. For example, two of the most interesting chapters in the volume are Lashley's study of *Nervous Mechanisms in Learning* and Hunter's *Experimental Studies of Learning.* The first concludes that we want a more definite descriptive psychology to build physiological theories upon, and the second that we want more definite physiological investigation to build theories of learning upon. No doubt both may be true. The general level of the contributions is high and an immense amount of information is crowded into the pages of this volume. There are two English contributors: H. Banister who writes on Hearing, and H. Hartridge who deals with the physiological side of the same topic. Prof. Carl Murchison has once more done an extremely good turn to psychology by the enterprise and skill he has shown in organizing and publishing this book. He speaks in his Preface of succeeding editions. It is greatly to be hoped that the public demand for this book will render these speedily possible and profitable.

Instinct and Intuition: A Study in Mental Duality. By G. B. Dibblee. London: Faber and Faber. 1929. Pp. 394. 25*s*. net.

Mr Dibblee is already known to psychologists for his very interesting and lively study of *The Psychological Theory of Value*. Approaching a study of the principles of Demand in the realm of economics, he was led to a critical examination of the notion of Value. This, again, led him to consider how often the fully formulated judgments of reason are set aside, or overborne, by judgments based upon no consciously realized factors And this naturally and justly led him to take up his present problems in *Instinct and Intuition*. To Mr Dibblee instinct is a mode of intelligence on one side of reason, and intuition a mode of intelligence on the other side of reason. It is not very easy to disentangle from his general discussions brief and adequate definitions, but this is what he says: "Instinct is a mode of intelligence and its function is to act in judgment on sensations and to pronounce on their merit and validity." He regards instinct as "common to men and animals." "Intuition is a mode of intelligence unconsciously exercised by man only among the animals." With a wealth of reference and illustration the author elaborates these definitions, searching for the physiological basis of both intuition and instinct, and discussing in detail their probable interrelations. He has been very strongly influenced by the work of Sir Henry Head and Sir John Parsons. The physiological side of the book is certainly not its strongest part, but the volume as a whole is worthy of serious attention. It is well written and interesting throughout and is full of valuable ideas.

Pleasure and Instinct. By A. H. B. Allen. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1930. Pp. ix + 336. 12*s*. 6*d*. net.

This is a volume in the International Library of Psychology, Philosophy and Scientific Method. It is a very thorough piece of work, dealing with a wide range of problems. Sensory feelings are first discussed. It is maintained that "pleasure is the mental element which accompanies the sensations of the bodily processes when they proceed in a normal fashion." Ordinarily slight and almost unnoticed it may, within certain limits of organic activity, be greatly raised by special stimulation. Unpleasure occurs as the result of a conflict between the impulse, "towards exercise and enhancement of the vital activities, and tendencies towards the depression and stoppage of vital functions, when that conflict reaches a certain stage of acuteness." This theory is fully worked out, though without first-hand experimentation, and alternative views are considered and criticized. The third and chief part of the book discusses the relation of pleasure and unpleasure to the main instincts. The instincts considered are those of nutrition, of reproduction, of curiosity, of mastery, and of altruism. All normal progress of conative trends tends to be pleasurable, but frustration of instinct, provided the instinctive drive continues to operate, results in unpleasure. Sometimes the mere drop of an organism to a lower level of activity, organic or mental, itself gives rise to pleasure. There are no neutral feelings.

These are only a few of the many topics discussed. The book is a sober and serious contribution, not very lively to read and not containing any startling originality. Yet it makes a genuine effort to tackle extremely difficult problems, and it must be held to have succeeded very well indeed within the limits of its method. The author wields no tools of experiment, but he has obviously thought much and sincerely about his problems. He knows the literature well and presents both his own and opposing views fairly and clearly.

The Process of Human Behaviour. By MANDEL SHERMAN and IRENE CASE SHERMAN. London: Williams and Norgate, Ltd. 1930. Pp. 227. 12*s.* 6*d.* net.

In this book chapters III and IV, dealing with *The First Human Responses* and *The Relation of Sensori-Motor Development to the Growth of Intelligence,* and chapters V and VI, dealing with *The Observation of the Emotions* and *The Nature of the Emotions and their Influence upon Behaviour,* are very largely the outcome of first-hand experimental and observational work. They are clear and definite and, in particular, the chapter recording and photographically illustrating early emotional responses is an excellent piece of work. The first two chapters deal equally clearly with the growth of the nervous system and its functional significance. Here the authors have drawn upon sound authorities and have selected well, though naturally the major part of the story remains untold. The last two chapters discuss the development of personality, considered as a system of individual behaviour, and with the relation of personality to social behaviour. These are scrappy and slight, though they are consistent and in a general way full of common sense. The whole book is very easy to read, but is at the same time accurate as far as it goes.

The Influence of Habit on the Faculty of Thinking. By MAINE DE BIRAN; trans. by MARGARET D. BOEHM. Introduction by GEORGE BOAS. Psychology Classics, vol. III, edited by KNIGHT DUNLOP. London: Baillière, Tindall and Cox. 1929. Pp. 227. 22*s.* 6*d.* net.

Maine de Biran's essay has never been put into English before, but, in view both of its intrinsic merits and of the influence it has had upon French psychological thought, it is very justly included in this important series. The translation reads well and is good. The brief introduction by Dr George Boas is very much to the point, and adequately describes the setting of the essay. Of the essay itself it is unnecessary to say much here. It is full of acute and interesting observation and, in days when more and more psychologists are becoming preoccupied by problems of learning, it should receive the warm welcome which it deserves.

Adaptability to New Situations. By S. R. LAYCOCK. Baltimore: Warwick and York, Ltd. 1929. Pp. xi + 170. $2.40.

This volume (Educational Psychology Monographs, No. 26) gives a clear and readable account of a number of experiments carried out by the author under the direction of Prof. C. Spearman. The experiments were carried out upon school children, and their interest lies largely in their attempt to provide situations and problems attractive to the candidates, and in the way in which the problems set by the initial tests were followed up by further experiment in an attempt to get at their solution. All the results are interpreted directly in terms of Spearman's *Nature of Intelligence and Principles of Cognition.* Prof. Spearman himself contributes a foreword to this book.

Social Psychology. By BERNARD C. EWER. New York: The Macmillan Company. 1929. Pp. viii + 436. 10*s.* net.

There is a definite value in books of this kind, which do not pretend to be original contributions to a subject, but have as their aim the simple presentation of comparatively well-established material. It would serve admirably as a text book to use with a class beginning the subject; but it is unfortunately bulky, and the style is commonplace. The author has, however, with respect to many problems, a refreshing sanity. The book contains three parts of which the first deals with *Fundamental Principles,* the second with *Individuality,* the third with *The Social Order,* and the fourth with *Systems of Social Behaviour.* Questions and exercises are appended to each chapter.

Studies in Service and Self-Control. By HUGH HARTSHORNE, MARK A. MAY and JULIUS B. MALLER. New York: The Macmillan Company. 1929. Pp. xxiii + 559. 12*s*. net.

'Service' means working for other people instead of for oneself, while 'self-control' contrasts "the tendency to continue an approved act with resistance to the tendency to engage in an interesting but disapproved act." Both are regarded as important character traits and their study and, if possible, their objective assessments are pursued with the utmost enthusiasm by means of the application of a bewildering variety of tests. The authors and their subjects alike must have got a great deal of fun out of these tests, some of which seem to run to the extremes of ingenuity. The authors got a great deal more as well, and state their conclusions in this attractive but somewhat wordy book. The most important general conclusion is that any battery of tests which might properly claim to measure any single type of conduct must include a large number of tests representing a variety of life situations. The detailed conclusions are much too numerous to be summarized here: there are thirty of them relating to 'service' and another eight concerning 'self-control.' Taking it all in all, the volume is decidedly an attractive one, and represents an account of pioneer work which may well develop into a matter of great importance.

The Real Rhythm in English Poetry. By KATHARINE M. WILSON. Aberdeen: The University Press. 1929. Pp. vi + 171. 7*s*. 6*d*.

As every psychologist knows, the topic of rhythm has an immense literature. A large amount of this can be safely avoided, but this is a book that nobody who is interested in the subject should neglect. It is rather brilliantly written; it has important things to say, and it has the courage not only to propound a somewhat unpopular theory but also to illustrate it abundantly and with unfailing relevance. The general view adopted is that rhythm is not primarily perceptual, not mainly a matter of affective response; but above and beyond everything else an experience of movement. The particular attempt is "to collate the prosody of poetry with that of music." This attempt is made in so delightful and yet so terse a manner that further summarization could only spoil its force. The book is emphatically one that should be read and studied. Everybody with a lively enough mind and some interest in the topic will enjoy it, and whether he agrees with it or not will learn much from its study.

Nouveau Traité de Psychologie. Par GEORGES DUMAS. Paris: Félix Alcan. 1930. Pp. vi + 425. 74 fr.

This is the first volume of a proposed complete psychological treatise which is to cover the whole field of normal and pathological psychology. Seven volumes are projected to deal with normal, and two with abnormal, psychology. The general subjects of this first work are the neurological, biological and physiological bases of psychology, and a study of psychological methods. Each chapter is the work of a specialist in its subject. Thus: R. Perrier deals with *The position of man in the animal scale*; P. Rivet with *The data of anthropology*; Ch. Champy with *The physiology of age and sex*; Louis Lapique with *The general physiology of the nervous system*; Aug. Tournay with *The special physiology of the nervous system*; Henri Wallon with *The biological problem of consciousness.* On the methodological side the editor writes *An Introduction to Psychology*, which deals lucidly with some of the main historical sources both in an experimental and in a general sense; while André Lalande writes on *Psychology: its various aims and methods.* For the general student the book is overloaded with detail. For the teacher, or for the student who is beginning to research in any of the special fields dealt with, the volume will be of the utmost value. There are fairly good selected bibliographies to each chapter.

L'Année Psychologique, 1928. Vols. I and II edited by HENRI PIÉRON. Paris: Félix Alcan. Pp. xvi + 945. 240 fr.

L'Année Psychologique is nowadays chiefly valuable for its original articles. These are almost always of a very high standard and full of suggestions for new lines of work. The present volumes contain the following studies:

The problem of successive visual impression in relation to the perception of movement, by G. Durup; *A contribution to the study of the factors which regulate the rate of summation of luminous impressions from surfaces of varied luminosity in different regions of the retina*, by N. Kleitman and H. Piéron; *Internal inhibition of fixation*, by M. Foucault; *A French standardisation of the Barcelona test*, by Mme H. Piéron; *The problem of very brief tactile stimulation*, by A. Fessard; *The influence of the duration of sounds on their timbre*, by H. D. Bouman and P. Kucharski; *Temporal principles determining the chroma of luminous sensations*, by H. Piéron; *The language and 'articulations' of thought*, by D. Bertrand-Barraud. In addition there are notes on *Phenomena of contrast in the combination of heterogeneous fields in binocular vision*, by H. Piéron; *The influence of the composition of light on the nature of Benham's colours*, by H. Piéron, and a description of new visual apparatus and of a modified Mile's Pendulum, by the same author. The bibliographical analyses comprise 1380 titles, and in many cases, as usual, contain excellent brief critical remarks.

Introduction à la Psychologie Collective. Par CH. BLONDEL. Paris: Armand Colin. 1928. Pp. 211. 9 fr.

Dr Blondel is convinced that social psychology has a field of study which is peculiarly its own, and further that many of the problems of general psychology can be solved only by a more thorough recognition of the peculiar contributions that can be made to them by social studies. He writes this excellent little work to justify and clarify his view. The first part of the treatise describes the views put forward about social psychology by Comte, Durkheim and Yarde, while in the second part he shows how social factors may influence perceptual reactions, memory and the life of feeling.

Anfänge der Reifezeit, Ein Knabentagebuch in psychologischer Bearbeitung. Von W. STERN. Leipzig: Quelle und Meyer. 1929. S. vi + 125. M. 2.30.

This is the second edition of Prof. W. Stern's already well-known studies in the trends of human development during the early teens. It is a condensed but excellent and important contribution based throughout upon first-hand records and observations. After a brief explanation of his methods Dr Stern deals with his problems under three main heads: the changed attitude of the individual towards his own unique individuality, the new and developing reactions towards other people, and the widening fields of interest, all of which characterize the growth towards adolescence. Each problem is pursued in a number of special topics, and about them all Prof. Stern has something valuable to say. The book is one that merits wide and careful attention.

Geniale Menschen. Von ERNST KRETSCHMER. Berlin: Julius Springer. 1929. S. vii + 253. RM. 12.

Whether he agrees with Kretschmer's psycho-biological approach to the study of men of genius or not, every reader must admit that this book is one of strong interest. The first part of it deals with principles. Here are discussed that force often apparently from the outside, which may seem to impel the genius to expression; the 'drive' (*Trieb*) hidden in his own personality; the typical stamp of individuality which he possesses; his mode of disciplining his abilities, and the relation of genius to biological inheritance. The second part builds up a picture of the man of outstanding gifts in his various typical forms, the artist, the investigator, the hero and the seer. Many topics are here considered: periodicity of expression, the flowering of special gifts at particular ages, the character of inspiration. The third part of the book gives a number of extremely well-reproduced photographs of men and women of genius. Naturally all the interpretations are inspired by Kretschmer's special views about the psychology of types, but it is a thoroughly interesting book with a strong general appeal.

Die Psychologie der Productiven Persönlichkeit. Von Paul Plant. Stuttgart: Ferdinand Enke. 1929. S. 332. RM. 15.

This book casts a wide net. It is very largely historical, and is concerned on the whole more with ideas and principles than with concrete studies. Rather less than the first third is devoted to a study of views about genius and intelligence. The author begins far back by a consideration of mythological roots in the notion of genius, and then deals with the development of the notion, among the Greeks, at the Renaissance period, in rationalistic philosophy, in the English, French and German aesthetic of the eighteenth century, and in Lessing and Kant. A couple of chapters on genius and madness, and on a general critique of the notion of genius round off this part. Intelligence is treated much more summarily and by reference in the main to contemporary German work. Here full allowance is made for the parts played by character and temperament in the direction of intelligent adaptations. The bulk of the work is concerned directly with the *productive Persönlichkeit.* A general introduction describes some of the modern methods of the study of personality, and this is succeeded by a treatment of the whole psychology of the matter. Artistic creation receives specially detailed consideration, but law, natural science, history, religion and literature all get their share of attention. The third part of the volume is concerned with views expressed by contemporary outstanding German workers in science and in art upon their own methods of work. These were expressed in response to a *questionnaire* circulated by the author and are in many instances of great interest. The whole book is undoubtedly one which all students on personality studies will find it profitable to read.

Versuch über Fühlen und Wollen. Von Johannes Volkelt. München: C. H. Beck. 1930. S. 131.

The problems of this book are ancient and highly controversial. Prof. Volkelt writes about them with engaging brevity and much force. First he deals with *feeling*, dealing with what 'feeling' means in common speech; 'feeling' as *Tustandlicheits-Innesein*; 'feeling' in relation to pleasure-pain, 'feeling' in relation to organic sensation; the relation of feeling and conation and the phenomena of feelings which remain in a rudimentary or incompletely expressed state. The discussion of willing follows the well-known path: desire, wish, resolve and execution. Willing is considered both as a positive demand and as a negative attitude, and finally the consciousness of the imperative is considered. The last section deals with *Bewusstseinsletzeiten*. First this notion of 'enjoyment consciousness' is discussed and then its applications to emotional and cognitive phenomena are considered. The conclusions are summarized at the end of the volume.

A Hindustani Binet-Performance Scale: with a Comparison of the Intelligence of certain Caste Groups in the Panjab. By C. Herbert Rice. Princeton: Princeton University Press (London: Humphrey Milford). 1929. Pp. x + 196. 9*s.* net.

The Stanford-Binet scale, certain Otis tests and the American Army Alpha tests were translated into Urdu, and other tests from the U.S. Army Beta, the Pinkner-Patterson Performance scale and the Porteous Maze scale were similarly treated. The tests were tried and then modified for use in the Panjab. In Part I of the present book are published the scale as used, a list of instructions, tables of norms, and a list of materials. Part II gives the Panjabi modifications and the result of their application to 929 Panjabi schoolboys. In Part III there appear the results of a comparative study of boys from the 'depressed classes' with other major caste groups of the province. The work was evidently done very carefully and is exceedingly clearly presented, so that the volume forms a valuable contribution to comparative psychology. At the same time it is rather a pity that the author did not show himself more fluid in his attitude to the test material, and try the effects of *ad hoc* tests constructed with particular reference to the social backgrounds of the various groups dealt with.

The Child's Conception of Causality. By JEAN PIAGET. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1930. International Library of Psychology, etc. Pp. viii + 309. 12*s.* 6*d.* net.

Prof. Piaget here applies his well-known method to an investigation of the child's views concerning: (1) explanation of movement; (2) prediction and explanation; (3) the explanation of machines. A final section sums up Piaget's beliefs concerning the nature of children's thinking with special reference to the conceptions of reality and causality. A large number of concrete topics are discussed, *e.g.* experiments concerning the nature of air; the origin of wind and of breath; the movement of the clouds and of the heavenly bodies; water currents and movements due to weight; the child's idea of force; the floating of boats; the level of water; the problem of shadows; the mechanism of bicycles; the steam engine; trains, motor-cars and aeroplanes. The general conclusion that children's thinking is in many important features essentially different from the adult's remains the same as in Piaget's earlier volumes. And once again the author has produced an original study of great interest and importance, rather diffuse in style, and rather biassed by the uncritical acceptance of current views with regard to the modes of thought of most grown-up people.

Problem Tendencies in Children. By W. C. OLSON. University of Minnesota Press. 1930. Pp. xi + 92. $2.00.

This is largely a study in method. Dr Olson and his collaborators keep two types of record for all the children whom they study. The first tabulates specific overt acts of misbehaviour, the second a rating of personal traits by an observer who is skilled and appears trustworthy. These two records are then related statistically and the final score serves as a "problem tendency indicator." It is perhaps not surprising that the author found that probably "all children are problem children, but that they are so in varying degrees." However, as an interesting bit of pioneer work, the monograph is of value. The methods used are carefully described so that they can be applied by any reader, and the results so far secured are candidly set forth and discussed.

The Language Development of the Pre-School Child. By DOROTHEA A. MCCARTHY. University of Minnesota Monograph Series, No. IV. 1930. Pp. xiii + 174. $2.00.

Records were kept of the running conversation of 140 pre-school children of Minneapolis. These were studied from the point of view of the length of the responses, their functions, their construction, and their expression in parts of speech. The result is an undeniably valuable contribution to the topic of language development in the child. An account is given of earlier work, and there is some discussion of general problems. The presentation is throughout clear-cut and definite, and the whole work deserves much praise.

The Process of Learning. By CONSTANCE BLOOR. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1930. Pp. vii + 279. 7*s.* 6*d.* net.

The sub-title of this book runs: *Some Psychological Aspects of Training and Discipline in School,* and the volume is addressed primarily to Training College students. Written in a simple and lucid manner, it gives an excellent account of learning processes, both on their more technical sides and as regards general character training. The book makes no particular pretence to be original, but it is capable and should admirably fulfil its purpose.

The Teacher's Many Parts. By Sir JOHN ADAMS. University of London Press. 1930. Pp. 362. 6*s.* net.

The book contains twenty lively short essays about how a teacher may come into touch with the public, with the school, and with himself. The essays—which have such titles as: *The teacher as human being,* as *psychologist,* as *disciplinarian,* as *humorist,* as *scholar,* to take a few at random—contain much wit, no small amount of good advice, and a deal of excellent observation and common sense. They are eminently easy to read, a little bit 'superior' at times perhaps. They will certainly entertain the teacher, and even more, perhaps, his scholars; and, as they can hardly fail also to do the teacher good, it is to be hoped that they will be read widely.

Animism, Magic and the Divine King. By GERGA ROHEIM. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1930. Pp. xviii + 386. 21*s.* net.

The topics dealt with in this book are (1) Animism and the Other World; (2) The Psychology of Magic; (3) The Medicine-Man and the Art of Healing; (4) The Divine King; (5) The Scapegoat. Under these heads a very large number of customs come up for review. The problems are various but the solution is always the same: primitive society and everything in it is organized on a basis of castration anxiety, and the whole history of humanity is a succession of phases of genital organization followed by a flight away from this, followed by a further recoil that lands humanity in some form or other of its 'primal phase' again. The number of phallic symbols and substitutes that appear in this book is absolutely dazzling, and the discussion may be cordially recommended to anybody who desires to appreciate the dogmatic lengths to which this sort of interpretation can go.

Die Psychologie der Schizophrenen und ihre Bedeutung für die Klinik der Schizophrenie. Von CARL SCHNEIDER. Leipzig: Georg Thieme. 1930. S. xi + 301. M. 23.

This is a detailed and pretty well documented study of dementia precox. The first part deals with behaviour symptoms, the second offers a detailed analysis of the clinical picture of the disorder, and the third discusses practical problems of importance in general treatment.

Therapy of Personal Influence. By EDWIN HOPEWELL-ASH. 1929. Published privately at BM/ELHA, London, W.C. 1. Pp. 96.

This little practical book is intended solely for the use of the medical practitioner. It gives clear and simple instruction in the carrying out of certain psychotherapeutic methods. The author very properly stresses the importance of the personality factor in regard to method of treatment, and gives a number of case studies. The descriptions of what the writer calls the method of "personal influence and suggestion" are adequate, so that they could be readily followed by anybody. But the accounts of hypnosis and of 'psychomagnetic methods' are too scrappy to be of much service. And, what is a much more serious blemish, the book shows no appreciation whatever of the psychology which underlies all suggestion treatment.

The Constitutional Factor in Disease. By ARTHUR F. HURST. London. 1927. Pp. 93. *The Troubled Conscience and the Insane Mind.* By CHARLES BLONDEL. London. 1928. Pp. 91. *Medicine and the Man.* By MILLAIS CULPIN. London. 1927. Pp. 67. *Hypochondria.* By R. D. GILLESPIE. London. 1929. Pp. 104. All published by Kegan Paul, Trench, Trübner and Co., Ltd. in the Psyche Miniature Series. 2*s.* 6*d.* net each.

Dr Hurst's essay is primarily of interest to the medical student and practitioner, but psychologists who are concerned with questions of temperament may find in it some relevant material. Dr Charles Blondel's two essays are preceded by a vigorous introduction by F. G. Crookshank. Broadly Blondel's thesis is that all goes well when a man can directly formulate his experiences, but that in insanity or in some forms of psychological disease the verbal formulations all get tacked on to the wrong experiences. The case is put clearly and persuasively but, so far as these essays go, in general terms only. Dr Millais Culpin's essay is admirable for the general reader, and also should serve well to forewarn the medical student of the value of a psychological point of view. His two chapters on *What is a neurosis?* and *What is behind the neurosis?* should be widely read. Finally Dr Gillespie writes in an engaging manner about hypochondria, the history of its study and treatment, its symptoms, and its psychopathology.

Critique of Love. By FRITZ WITTELS. London: George Allen and Unwin, Ltd. 1930. Pp. viii + 313. 12*s.* 6*d.* net.

The author regards this as an 'original study' in the application of psychoanalysis to love. After a brief but interesting account of the way in which Freud arrived at his methods and views, Mr Wittels takes up a number of different topics and tries, in a not unfamiliar way, to show how love motives may lie behind an immense diversity of human conduct. In the course of his discussions he deals with sex deviations, masked sadism, bisexuality, narcissism, parents and children, *le grand amour*, great haters, love and marriage and the childwoman. The writing is in a popular vein throughout, with many illustrations. In point of fact the volume is not really very original except in its application of well-known principles of explanation to new instances. The instances, also, though new in detail are of the type that have been dealt with in this way many times already.

The Psychology of Religious Adjustment. By EDMUND S. CONKLIN. New York: The Macmillan Company. 1929. Pp. xiv + 340. 8*s.* 6*d.* net.

This is a quiet and in many ways unusually helpful study of psychological reactions in the religious realm. All the usual questions are discussed and particularly in the study of various forms of religious exercises there is considerable successful attempt at first-hand concrete study. The author is well acquainted with the literature of the subject and can be relied upon for a sane and considered judgment. He is also successful in maintaining a properly psychological point of view throughout.

The Pathetic Fallacy. By LLEWELYN POWYS. London: Longmans, Green and Co. 1930. Pp. 129. 5*s.* net.

This is not a psychological book. It is a delightfully written and produced brief history of Christianity from its origin to the present day. The author is appreciative, but sees Christianity as "a dream that has taken possession of the human imagination for two thousand years....Something of it will remain for all time, mystical and persuasive like the smell of summer rain on a parched garden plot. The religion as we know it will surely drift out of our thoughts." It would be interesting to know a little more about what will remain, but here Mr Powys is not very definite.

Man and the Image of God. By H. M. FOSTON. London: Macmillan and Co. 1930. Pp. vii + 228. 7*s.* 6*d.* net.

On the face of it it would hardly appear that this book could have much to do with psychology. But Dr Foston is obviously well-read in and appreciative of the more theoretical sides of the subject and his treatment throughout has much of psychological interest. In the beginning it is based, rather dangerously, upon an analogy between the Doctrine of the Trinity and the conventional tripartite classification of ultimate mental reactions into knowing, feeling and striving. That the validity of the latter should strengthen belief in the former demands, obviously, other assumptions which not everybody will be willing to make. The author is not unaware of this and, in what he calls 'Critical Review,' attempts further justification of his view that man inevitably bears in his nature the "image of a transcendent Triunity." In the rest of the book he works out what he regards as the consequences of this position, especially in regard to the Creation (*i.e.* the general outer environment), the Incarnation, and the place of Love in the universe. Whether the whole essay is regarded as in any way convincing or not, it is a valiant constructive effort, and a psychologist with some imagination and a *flair* for speculation will enjoy its reading.

Five Types of Ethical Theory. By C. D. BROAD. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1930. Pp. xxv + 288. 15*s.* net.

The five types dealt with are represented by the theories of Spinoza, Butler, Hume, Kant and Sidgwick. Dr Broad gives brief biographical details of each of these, and then in turn submits their theories to his most searching and critical examination. Naturally there is a good deal of psychological argument in the course of the book, as in the sections dealing with psychological egoism, conscience, psychological hedonism and the like. This is all extraordinarily clear and decisive. To Dr Broad no one of these five types is wholly satisfactory, and his general conclusion is that the whole subject of human desire, emotion and action is one of extreme complexity, and that man is in a paradoxical position "half animal and half angel." To this, no doubt, the psychologist will assent without difficulty. It would, however, be most unfair to suggest that this is the sum of the whole matter. The destructive criticism is often lit by brilliant positive suggestions, and in his final conclusions Dr Broad puts forward several notions which justly demand much further study. Especially the distinction between the total goodness *of* the community and total goodness *in* the community is one which every social psychologist will want to know much more about.

Let whoever reads this book study also the Index.

Mind and the World-Order. By C. I. LEWIS. London: Charles Scribner's Sons. 1929. Pp. xiv. + 446.

This exceptionally able and well-written book contains much that will repay the serious study of every epistemologically inclined psychologist. Naturally it is largely concerned with problems in the perception of external objects and with an appraisement of scientific concepts. Its bent is throughout constructive. The author, perhaps a little unfortunately, labels his view 'conceptualistic pragmatism,' but, as he himself remarks, most pragmatists would give it only a very qualified allegiance.

Your Character from your Handwriting. By C. HARRY BROOKS. With a foreword by ROBERT SAUDEK. London: George Allen and Unwin, Ltd. 1930. Pp. 159. 3*s.* 6*d.* net.

The contents of this book are sufficiently indicated by the title. It is a popularization of the work of Saudek, and contains numerous illustrative examples.

Some Aspects of Hindu Medical Treatment. By DOROTHEA CHAPLIN. London: Luzac and Co. 1930. Pp. 71. 3*s.* 6*d.*

This brief monograph, which is largely concerned with methods of psychotherapy, may be of some value to students concerned with comparative psychology. The accounts are mainly at second hand through the teaching of Mr S. M. Mitra. It goes without saying that the methods described must not be applied indiscriminately by persons having no general medical training.

The Nature of Consciousness. By E. R. ROST. London: Williams and Norgate. 1930. Pp. 158.

The author hopes that his work will help to bring "the study of Psychology within the range of Science." As a matter of fact it will be totally incomprehensible to any reader who is not well versed in Buddhistic ideas and willing to accept them without criticism.

VOLUME XXI JANUARY, 1931 PART 3

TESTS OF DISTRIBUTED ATTENTION: AN INVESTIGATION INTO THE DIFFERENCES BETWEEN TESTS WHICH APPEAR TO MEASURE CERTAIN ASPECTS OF DISTRIBUTED ATTENTION[1].

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BY F. M. EARLE.

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I. INTRODUCTION.

WHEN the psychologist enquires of the workshop manager or foreman what are the chief abilities needed in the successful pursuit of some occupation, he is usually told that, among other things, the ability 'to pay attention' is very important. Sometimes such a statement is further qualified by terms implying 'concentration' and 'distribution.' It is supposed that in certain kinds of employment it is necessary to be able to 'concentrate,' whereas in other occupations it is essential to 'attend to several things at once' with accuracy and despatch. And it is often believed that clear-cut types of 'concentrators' and 'distributors' exist.

But according to Spearman, both the intensity and extensity of cognitive operations (*i.e.* 'concentration' and 'distribution' of attention) depend upon a general factor '*g*.' "In so far as they do so, the two constitute *alternative dimensions of the same cognitive output* characterising each individual." Further, he says, "neither the intensity nor the extensity generates any kind of group factor. Although faculties for each of these have been implied and stated very widely, and although upon them have been based projects and even actions of grave moment for education and industry, still, according to our present experimental evidence neither faculty has any real existence[1]."

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[1] An investigation carried out at the National Institute of Industrial Psychology with the aid of a grant from the Laura Spelman Rockefeller Memorial.

Even if the theory of psychological 'types'—such as 'concentrators' and 'distributors'—be no longer held, a practical problem still remains. The manager, the foreman, the storekeeper, the sales' assistant, the nurse, the machine-minder, the bus-driver are all called upon periodically to maintain continuity of action in a number of processes over a considerable period of time. What special characteristics does the 'attention to duty' of each of these persons possess, and in what ways do the qualities of attention needed in one occupation differ from those of another? Have, for instance, the intensity and the duration of attention necessary for success in teaching a class of children any connection with the qualities of attention required in 'minding' a complicated piece of machinery? Can the psychologist discover any individual differences in regard to 'attention' that may be of service in vocational guidance?

The problem is a difficult one because of the complications of other factors. Interest in the activity cannot be ignored as one factor in success; but we may, of course, assume that whenever interest in an activity *already* exists attention will be given at least with the intensity necessary for its successful completion. Even so, whenever some bodily action has to be performed, the efficiency of the performance is affected not only by the 'attention' given to the task but also by the bodily 'skill' of the performer. Correspondingly, in mental processes, there is the intellectual ability of the individual to consider; can we say, for instance, that the intensity of the attention of the 'duffer' at mathematics is one whit less than that of the 'intelligent' boy who finds the subject easy? It may be that in purely intellectual processes intensity of attention and ability are inseparable, but in physical activities this does not seem to be universally true. When I make a stroke at tennis the result depends more perhaps upon my muscular dexterity than upon my attention; for if I play carelessly (*i.e.* at less than my customary intensity of attention) it seems possible for my acquired neuro-muscular habits to complete the stroke with reasonable efficiency. But, of course, this could not go on indefinitely.

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[1] Spearman, *Abilities of Man.*

In any experimental work on these problems it seems desirable, therefore, to eliminate (as far as possible) complicating factors such as special knowledge, bodily dexterities and the like. The tests described below were devised with these aims in view. The results, while differing little in some respects from those of other investigators do, however, bring out interesting points which may prove useful in any further enquiries into these problems.

The tests were devised with particular reference to:

(*a*) The measurement of the intensity of the attention of the individual to a single object (which is not too complex) by the efficiency of the resulting cognitions.

(*b*) The measurement of the ability of the individual to maintain a succession of attentions to a number of objects, independently of any automatisms needed for success.

Most tests of attention hitherto used have followed the method of (*a*). Attention is measured at its maximal capacity by any convenient task in which output is easy to control and in which maximal output may be expected. But tasks such as the common ones of card-sorting, cancellation, etc., depend partly upon the manipulative dexterity of the individual, while the instantaneous exposures of the tachistoscope are not sufficiently like the practical procedures of everyday life to give much help in problems arising out of these. In the tachistoscope experiments, according to McQueen, the 'persisting image' explains success[1]; but although this plays a prominent part in everyday activities, in these the *duration* of exposure is much longer than in the tachistoscope. To measure attention satisfactorily we really need some activity which allows attention to express itself freely *without time limit*, even although speed of cognition be used as a partial measure of the efficiency of the attention. Provided that the object of attention does not require any special knowledge to interpret it, the speed as well as the accuracy of a particular cognition should give a fairly satisfactory measure of the intensity of attention by which it is gained.

In (*b*) we have an aspect of the problem which seems to have received less examination than it deserves. There are, of course, many forms of reaction-time apparatus which require a distribution of attention. In a multiple-choice reaction-time test, however, attention is usually directed by the unpractised subject more to the selection of appropriate response than to the stimulus itself. Certainly the stimulus, like the starting signal of a race, comes during a period of intense attention, but the distribution of attention varies greatly as between the stimulus and the response, and, as is well known, the speed of reaction varies accordingly. In a more practical activity such as driving a motor car in which similar conditions prevail, although attention must also be distributed between the various stimuli and the appropriate responses, there is seldom need for the highest speed of response. The reactions required are hardly ever so sudden that they need to be measured in thousandths of a second. Each response as it occurs comes after a period of preparation (long or short according to circumstances)—there is always an anticipation that, in a moment or so, this or that movement of lever or wheel will have to be made. Moreover, in the skilled driver the purely manipulative processes are purely automatic; brake levers do not require searching for. Hence, given automaticity in the operation of hands and feet, the efficiency of the motor driver's performance depends almost wholly upon the clearness with which sense impressions are received and interpreted. Even in emergency, when accident is avoided by a hair's breadth, this remains true; the best driver is not one who overlooks (deliberately or otherwise) the significance of the events leading up to the 'tight corner,' although he may succeed in getting out of it by quick thinking and rapid action. Generally the quick response of the good driver is an indication of his 'preparedness.' In practical situations where inefficiency or 'flurry' occurs, an explanation may be found in the failure of perception; important signs will be overlooked or their significance perceived too slowly, and the result is a congestion of mental processes and an imperfect response. This slowness of perception may be due to the inability of the individual to attend to several things quickly enough, to a lack of experience preventing him from understanding the significance of a particular sense impression (the failure of the novice), or to emotional excitement. The importance of securing an equality of experience and knowledge in the processes to be used as a test of attention seems obvious. Emotional excitement and failure of perception may, however, be closely associated; for the latter may be one of the ways in which a tendency to 'excitement' under the most ordinary circumstances expresses itself.

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[1] McQueen, "The measurement of attention," *Brit. J. of Psychol.* Mon. Supp. 1917, v.

Hence the first of the following tests was designed to reproduce conditions in which attention could be given at high intensity over a narrow range, the second to provide a situation in which attention could be given successively (*i.e.* distributed) to a number of objects within a comparatively short time[1]. In both cases the objects were to be apprehended visually, this being the form of perception usually employed in everyday activities. The manipulative elements were reduced to a minimum and there was little or no need for any previous experience or knowledge.

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[1] Short as compared with the duration of ordinary activities, long as compared with the exposures of the tachistoscope.

II. DESCRIPTION OF TESTS USED IN THIS ENQUIRY.

Test I. A test of speed and accuracy of perception.

The apparatus used in this test was as in Fig. 1. It consists of a large wooden disc 12 in. in diameter and ½ in. thick, enclosed in a shallow box in such a way that it rotates easily about a central axis. The disc is so arranged that part of it projects sufficiently beyond the edge of the box to enable the experimenter to move it easily and quickly. In the cover of the box a rectangular hole 2 in. × 1 in. is cut in such a position that, as the disc is rotated, the diagrams pasted on its surface may be successively brought into view.

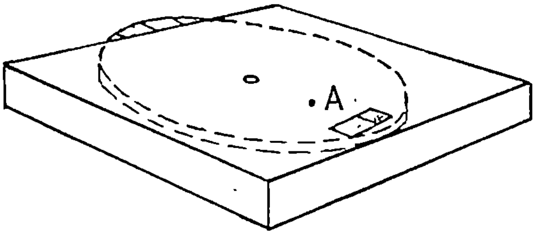


Fig. 1.

The standard diagrams were like this:

XVIII		XVIII
Poor		Cool
ɵɵɵɵɵɵɵɵ	*or*	ɵɵɵɵɵɵɵɵ
ɵɵɵɵɵɵ		ɵɵɵɵɵɵ
ɵɵɵɵ		ɵɵɵɵ
ɵɵ		ɵɵ
food		moon

It will be observed that the elements consist of:

(i) A Roman numeral.
(ii) An adjective and a noun forming the phrases 'poor food,' 'cool moon.'
(iii) A symmetrical arrangement of 'modified' O's (obtained by superposing two typewriter letters such as O and o; O and e, etc.).

The two diagrams shown above were pasted on the outside of the box (at A) near the aperture at which the diagrams on the disc were successively exposed. Forty similar diagrams were pasted on the disc so that they were clearly visible at the window as the disc was rotated. Half were like the models; the incorrect diagrams were formed by altering:

(*a*) The shape (but never the symmetrical arrangement) of the O's; thus Θ ϴ.

(*b*) The combination of adjective and noun in such ways as 'cool food,' 'fool moon,' etc.

(*c*) The Roman numerals.

In a few of the incorrect diagrams all three errors were introduced, in others two errors, and in some one error only was made.

The subjects were seated comfortably, in such a position opposite the experimenter that the aperture and the correct models could be clearly seen. The experimenter then exposed, one by one, six trial diagrams, explaining that some were correct and some incorrect, and pointing out exactly in what respects the incorrect ones differed from the models. In these examples the three types of error were illustrated, and it was explained to the subject that he would be required to pick out similar errors in other examples. The subject was then asked if he understood clearly what he had to do and, as a precaution, the six diagrams were again successively exposed so that the subject could respond to them in the proper manner. Forty diagrams were then successively exposed. The responses of the subject were noted in detail, showing how many of the elements were correctly observed. The experimenter himself operated the disc so that the subject had only to watch the diagrams and make the appropriate responses. The subjects were asked to point to the errors, and not merely to mention their number. They were also advised to look carefully before responding, but to do so as quickly as possible. Hence, as soon as the subject gave his judgment, the experimenter passed on to the next diagram, and then noted the result of the previous exposure during the time the subject was examining the new diagram. At the end of the series the total time spent by the subject on the task and his total errors were recorded.

The detailed instructions for giving the test are as follows:

Show the subject the apparatus and demonstrate how the disc may be operated.

"Here is a box with a wheel inside it which can be turned round like this. [Demonstrate.] On the wheel there are a lot of little drawings on which there are also words and numbers. By turning the wheel these drawings keep appearing at the little window here. [Point.]. Do you see?

"Now here is the drawing you have to look out for. [Point to sample pasted on the board.] Notice how it is arranged: notice also the words. Both sets of words are right—both 'poor food' and 'cool moon'; that's why we have put two samples for you to look at.

"Now here [point to first diagram of the series now exhibited] is a picture which is just like the sample one. You see that in every detail it is like the one you have to look for. Now I turn the wheel round. Is the next one a correct copy? Yes, it is a correct copy. The two diagrams are alike. And the next? No, that is not a correct copy. [Point out errors.] And the next? Again it is not a correct one. [Point out errors.] And the next? Yes, that is correct. And the next? Once again the diagram is incorrect. [Point out errors.]

"Now you see how it works. Some of the diagrams are right and some of them are wrong. Well, what I want you to do, is to watch as I turn the wheel round and say 'correct' if the diagram is quite correct but point out the mistakes if there is anything wrong. Do you understand?

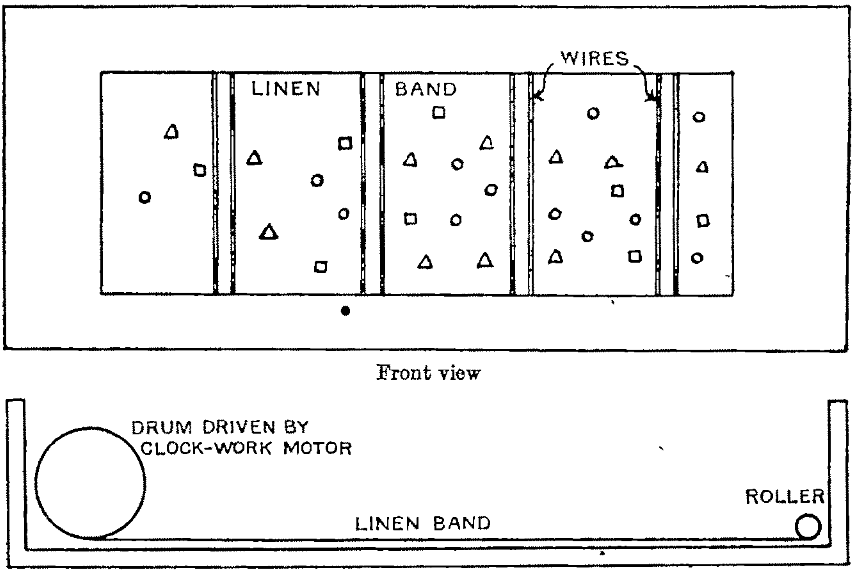
"Now we will try the first six diagrams again, just for practice. Are you ready? Yes, that's good. Now we will start. As each diagram comes round, I want you to say whether it is a correct copy or not. Don't take too long over each one. Just satisfy yourself which it is as quickly as possible and then tell me."

The kind of attention required in this test is exemplified in the 'inspecting' processes in the factory. The worker passes or rejects the finished product according as it does or does not equal the standard. This process is similar to that of searching (as in the cancellation test) for 'o's' or 'e's' in the lines of a printed page, except that the 'standard' is more complex, and several points may have to be looked for at one inspection. Consequently a fairly complex object must be used in this test, so that both coarse and fine errors can be introduced. Moreover, the manipulative element (as it enters in cancellation and similar tests) must be excluded. Success in the task, so far as it can be analysed, depends upon the intensity of attention during the exposures, and the accuracy and speed of perception resulting. Memory comes in only slightly because the models are constantly in view.

Since there are at least three items (or parts of the diagram) to be examined, it might appear that a distribution of attention also enters into the process. Certainly a rapid survey of the whole diagram is needed. But discussion of this point may conveniently be deferred until we have described the second test.

Test II. A test of distribution of attention over objects in a visual field large enough to require frequent and considerable shiftings of the eye's fixation point.

The apparatus (see Fig. 2) consists of a wooden screen with a window in it of dimensions 24 in. × 6 in. A long linen band 6 in. wide is made to travel slowly from right to left behind this window. On it are a number of coloured geometrical figures which appear in view on the right and eventually pass out of sight at the left. The rate of movement is comparatively slow, so that it is possible for the subject to traverse by eye the whole of the linen band exposed during the time that the figures are moving through only a short distance. Four pairs of black wires are stretched across the window at equal distances apart.



Front view

Plan

Fig. 2.

The space enclosed between each pair of wires is sufficient to allow one geometrical figure at a time to appear wholly within it. The figures used are squares, circles and triangles, of approximately equal area, and coloured green, yellow and red, singly and in various two-colour combinations. The figures which the subject is required specially to observe are respectively green triangles, red circles, and yellow squares. Yellow triangles, red squares, etc., are ignored. When any one of the selected three figures appears within any pair of wires a response has to be made. This response is of as simple a kind as possible. Plain cards are given to the subject, and he is asked to put a card in one of three boxes according to the figure to which he is responding. For example, the response to a red circle consists in placing a card in the red circle box, which is plainly marked by a large drawing of a red circle fixed above it. (It would, however, be better to employ three keys to register these responses.)

The general instructions for giving the test are as follows: The examinee is to be seated comfortably at the table on which the apparatus is placed. He should be at least 2 ft. (if possible 3 ft.) from it. The response cards are arranged in a pile on the table in front of the subject. He may take some in his hand if he wishes to do so. The three boxes in which these cards have to be put are placed directly in front of the apparatus and about 6 in. apart.

The general nature and purpose of the apparatus (including the mechanism—as a matter of interest and to avoid distraction due to curiosity as to 'how it works') are explained to the subject, and the motor is then started. The first two signals are responded to by the examiner, cards being placed in the appropriate boxes, in the exact manner in which the subject is required to respond. He is then asked if he clearly understands what he has to do. This being the case he is allowed to make the responses to the next signals in order to become accustomed to the rate of movement. The motor is again stopped and the subject is asked if he is ready, and then the command to begin is given when the motor is started for the third time.

The preliminary conversation usually takes this form:

"You see this set of drawings here. They are drawn on a sheet of linen which I have arranged in such a way that I can make it move slowly. [Show back of apparatus.] When I start the clock-work motor here, it wraps the linen sheet round this roller. Do you see? Now take a seat here so that you can see all the drawings. Is that comfortable? Now I want you to notice these black wires. There are four sets of them. [Point them out in turn.] Now as the linen band moves, the diagrams on it pass behind these wires, just as this one is doing now [point one out], so that at certain times a drawing will be exactly between one pair of black wires. Do you understand that?

"Now there are a great many drawings on the sheet but there are only three kinds that matter. You see them here [point to models on boxes]—a yellow square, a red circle and a green triangle. Do you see them? Say what they are? [A yellow square, etc.] And here are some plain cards. What I want you to do is to watch the linen band as it moves, and whenever one of these three drawings [point to them again and name them] comes right between any pair of wires you must pick up one of these cards [point] and put it in one of the boxes which you see here [point]. It must go into the box, the drawing above which corresponds in shape and colour with the drawing which can be seen between one of the pairs of wires. If a green triangle appears between any of the wires, then you put a card into the green triangle box. Do you understand? Watch me do the first two and then you can have a try. [Start motor and demonstrate with first two signals.] Now you try, this is only for practice. [Practice on next three signals.]"

The general instruction to respond only to the red circle, green triangle and yellow square is repeated, and then the experiment proper is begun. All errors are recorded.

The order of signals was so arranged that a response was required after an interval varying from 3 seconds to 17 seconds. The average interval was approximately 7 seconds.

Visual presentations were used in this test as being most easily arranged and most commonly employed in practical situations. The aim was to present within the field of vision a number of objects moving simultaneously across the field. In ordinary circumstances attention can either be directed to the movement of one object during the whole of its journey, or it may be given successively to different objects. In this test, however, the latter type of attention, *i.e.* successive attention to a number of objects, is compulsory, since no single fixation point will permit apprehension of the whole field.

The abilities required for success in this task appear to be:

(*a*) To remember the 'adequate' stimulus—red circle, green triangle and yellow square.

(*b*) To remember that different responses are required to each and to select the correct one. (But each signal suggests its own response.)

(*c*) To distribute attention over all the four pairs of wires (several subjects temporarily forgot one of the pairs).

(*d*) To establish a succession of attentions between the various competing stimuli.

Of these (c) and (d) appear to be the most difficult. Sooner or later the various irrelevant stimuli (such as all the other coloured drawings) must cease to receive any conscious attention; in other words, the intensity of attention for the three significant drawings must be sufficiently high to exclude all the others, but not so high that attention to one of the three significant drawings tends to prevent attention to the other two. This equilibrium of attention having been secured for the three drawings, they have then to be located as 'approaching,' 'half within,' or 'wholly within' one of the pairs of parallel wires. Suppose that one response has been made—there is now time to traverse the field visually to locate the position of the drawing which requires the next response (many subjects can go further and locate a succession of such drawings). Hence the next stage of the process, viz. the discriminative reaction, does not come without warning; there is usually sufficient time to locate the proper response box (in fact many subjects show their anticipation by selecting the appropriate response and holding themselves in readiness to carry it out the moment the signal arrives). To that extent, therefore, a subject's quickness of reaction to a given signal does not, within certain limits, influence the results. On the other hand, when a subject overlooks a signal and only notices it at the last moment, his response is apt to be flurried and this may affect him for several successive responses. But the time allowance appeared to be sufficient to allow the great majority to recover quickly even after the belated discovery of an overlooked signal. In this connection it is to be borne in mind that the test seeks to pick out those who get 'flurried' under comparatively simple conditions. It seems reasonable to suppose that such subjects would respond equally badly in more complex situations.

Differences between Tests I and II.

It would appear on analysis that the two tests differ in this respect:

Test I requires a high intensity of attention over a narrow range of objects (three or four elements in the whole).

Test II requires a lower intensity of attention over a wider range of objects (three objects, four positions, three reactions).

As regards the manipulative aspects of Test II, the reaction is so simple that it may be assumed to be 'automatic' almost from the start. This is true of cancellation tests also, but the relation between the time required to perform each act and the test as a whole is of some importance. In this test the proportion of time spent in actual manipulation is small. Attention is directed to locating the proper box for response rather than to picking up a card (because this is such a very common process and everyone can do it 'sub-consciously' or 'marginally'). Further, success in Test II does not depend on memory (*e.g.* remembering the kind of response), because each fresh signal for a response gives its own cue. In other words, the discriminative reaction is of the very simplest type and does not require the memorizing of a series of alternative situations and responses. Thus the verbal accompaniment (sub-vocal) of a response when it occurs might be expressed something like this. "Here's a red circle coming" (he picks up a card). "Where's that red circle box? There it is [moves hand to proper box]." "There it goes [drops card]." "Now where's the next one? There goes a triangle. No it isn't (colour is wrong, perhaps)." "Ah! Here's a yellow square!" [In many cases this attention to the next signal will take place as soon as the automatic process of dropping the card and picking up another one has been started on its way; in some cases it may even begin *before* this physical activity is allowed to begin.]

In the case of Test I, however, it is possible that memory for a complex diagram visually presented may be a contributing factor (cf. McQueen's 'persisting image'). Although the correct models are constantly in view (for purposes of reference when necessary), it was observable that many subjects do not refer to them at all after the correct form has been learnt. It was thought desirable, therefore, to compare the results of this experiment with those obtained from a test in which memory was more directly employed. For this purpose two supplementary tests were used. They were:

Test III. A test of perception of form in which immediate memory enters.

Forty different rectilinear diagrams were prepared resembling that given in the sketch (Fig. 3). Variations in them were made by changing the relative size of the sections, and by changing the positions of the diagonal lines and the cross. The diagrams were arranged in pairs so that some of the pairs consisted of two identical diagrams, while in others the diagrams were unlike in some way.

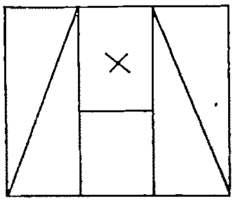


Fig. 3.

The procedure was as follows. One of the diagrams of a pair was exhibited for 10 seconds. It was then covered over, and the other diagram of the pair was similarly exhibited. The subject was required to say whether the drawings were exactly alike or, if not, in what respects they differed. (Suitable explanations and practice trials were first given.)

Such a test requires the clear perception of detail, whether or not the object is consciously analysed and noted. Unless this perception of the first diagram is accurate and clear, the exposure of a second diagram (which differs only slightly from the first one) will tend to produce uncertainty and doubt. (This uncertainty may also occur when the diagrams are identical, especially if the recall be at all imperfect; but in material of this kind it may be less difficult to recognize identity than difference.) It was observed, however, that a large proportion of the subjects examined were able to specify the points of difference with considerable accuracy.

Test IV. Another test of perception of form in which immediate memory enters.

In this test eight rectilinear diagrams of gradually increasing complexity (see Fig. 4) were exposed one by one for 15 seconds. After each exposure the subject was asked to sketch the diagram he had just seen in as correct shape and proportion as he could. Marks were awarded for correctness in reproducing the symmetry, proportions and details of the drawings, according to a fixed scale.

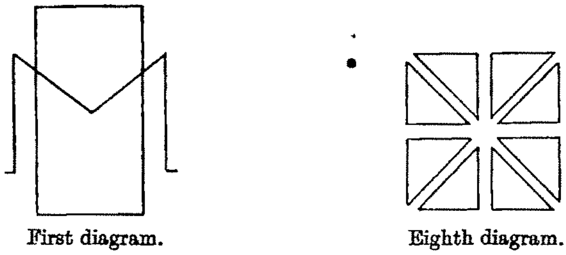

First diagram. Eighth diagram.

Fig. 4.

Confusion arises in this test chiefly owing to the vagueness of the images which are recalled. Since there is a symmetrical arrangement in nearly every drawing which an intelligent observer usually notes, and which serves as an aid to recall, it is to be expected that the results of the test will disclose a connection with what is commonly called 'general intelligence.' In other words, successful recall in this case seems to depend upon the 'organization' as well as upon the clearness and intensity of the perceptions involved.

Tests V–IX.

But immediate memory for a visual impression is not the only special factor which requires consideration in these tests. Consequently it was considered useful to compare the results with those obtained from the following additional tests.

Test V.	The Stanford Revision of the Binet Intelligence tests as revised for London children.
Test VI.	A Group Intelligence test, No. 34, devised by the National Institute of Industrial Psychology for use with elementary school children of age 13–14.
Test VII.	A test of Substitution (Woodworth and Wells)[1].
Test VIII.	A Maze test (Porteous)[1].
Test IX.	A Group test of Form Relations[2].

The importance of making a comparison between these tests and Tests I and II is apparent. We are concerned in this enquiry with the form of objects. It may, therefore, be helpful to observe how successful the subjects are in dealing with simple problems in which only the ability to perceive quickly and accurately relations of shape and form is required. In a test such as Test IX differences in capacity to remember do not enter at all, the forms and their parts being observable on the same page throughout the whole time allowed for the solution of the problem. Similarly, since the organization of experience, immediate or remote, contributes to the successful performance of the various tasks employed in the enquiry, some measure of intelligence is necessary. We were fortunate enough to be able to obtain measures of general intelligence from both group and individuals tests, and from both the so-called 'verbal' and 'non-verbal' types of test also.

The possible effects of imperfect vision upon the processes studies in these tests were also considered. As will be seen (p. 230) there were no cases of visual defect which seemed sufficiently serious to affect the general results.

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[1] Tests selected from the Scale of Performance tests standardized by F. Gaw for London School children. (*I.F.R.B. Report*, No. 31.)

[2] The Form Relations test consists of a number of geometrical figures so drawn that in each one a portion of the figure is shown to be missing. The subject is asked to choose from among a number of alternatives the 'part' which belongs to each incomplete 'whole.' The figures are arranged in sets of five, the sets being graded in difficulty and presented separately.

III. STUDY OF RESULTS OBTAINED.

All the children were of age 13 years 9 months, 13 years 10 months, 13 years 11 months. There were 37 boys and 41 girls. They were of elementary school education and training and were in their last term of school life. They were of working class parentage.

(1) *Sex differences.*

The following table shows the mean and median scores for boys and girls, with the differences between them and the probable errors of these differences:

Table I.

Test	Sex	No. of cases	Mean	Median	Sex difference between means	Probable error
I	Boys	37	10·76	11·0	1·35	±0·89
	Girls	41	9·41	9·5		
II	Boys	37	12·75	13·0	0·57	±0·7
	Girls	41	13·32	13·5		
III	Boys	37	12·24	12·0	0·16	±0·47
	Girls	41	11·64	12·0		

The scores are given as the number of 'errors' made. Maximum error score is 40.

On these figures the differences between the mean scores for boys and girls (which are in Tests I and III opposite in sign to those in Test II) are no larger than might be expected by purely chance circumstances. For this reason, whenever it was desired to compare the results of these three tests with others in which, similarly, no sex differences were observable, the above groups were combined into a single group of 78 cases.

The median and quartile scores for the combined group are given in Table II.

Table II. *Median and quartile scores for Tests I, II and III* (78 *cases*).

Test	10th percentile	25th percentile	Median	75th percentile	90th percentile	Mean
I	17	14	10	6	3	10·0
II	20	16·5	13	9·5	6·5	13·05
III	9	10	12	14·5	16·5	11·9

(2) *Relation of test results to visual acuity.*

The scores in Tests I and II were considered in relation to the quality of vision possessed by the subjects. The following results do not suggest that visual acuity produced any important effects. The ratings for vision were given by the visiting medical officer[1].

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[1] In connection with an experiment in vocational guidance carried out by the N.I.I.P.

Table III.

Visual acuity	Test I Score range	Test I Median	Test II Score range	Test II Median
$\frac{6}{6}$	0–23	10	3–22	12
From $\frac{6}{9}$ to $\frac{6}{12}$	2–21	12	2–25	16
From $\frac{6}{12}$ to $\frac{6}{18}$	2–16	7	7–23	15
Worse than $\frac{6}{18}$	0–17	10	9–24	14

There is clearly no direct correspondence between errors and quality of vision.

(3) *Relation between time taken and errors made in Test I.*

In Test I it was arranged that the subject should be allowed as much time as he required for the inspection of each diagram (though the general instructions emphasized that the inspection should be done quickly). The question arises whether the slower and more deliberate observers are more accurate on the whole than the quicker ones. The view that the steady plodder never does himself complete justice in a test in which there is a time limit is constantly advanced by those who are disposed to question the validity of intelligence and other tests. In the present case the results show that, so far as the abilities required for success in this test are concerned, there is no ground for this belief.

	Boys (37)	Girls (41)	Whole group (78)
Mean time taken to complete the inspection of 40 diagrams	266·4 sec.	*289·1 sec.	277·1 sec.
Standard deviation	72·3 sec.	104·9 sec.	90·5 sec.
Correlation with score ...	−0·38 ± 0·09	−0·44 ± 0·085	−0·37 ± 0·066

* Sex difference between means −22·7 sec. P.E. of difference ±13·6.

Although the correlation between the time taken and the total score is not large, it is significant and it indicates that there is a definite tendency for the more rapid observers to make the fewer mistakes.

(4) *Comparison between the results of different tests.*

In Table IV the inter-correlations between tests are given for boys and girls combined in those tests in which no sex differences appear. In Tables V and VI the sexes have been treated separately.

Table IV.

Test	No.	I	II	III	IV	V	VI	VII	VIII
Inspecting	I	—	·29	·41	·31	·41	·37	·30	·26
Machine-minding ...	II	·29	—	·24	·35	·26	·25	·13	·14
Pairing	III	·41	·24	—	·22	·27	·22	·27	·32
Memory drawing ...	IV	·31	·35	·22	—	·61	·58	·45	·44
Intelligence (Binet)	V	·41	·26	·27	·61	—	·81	·53	·47
Intelligence (Group)	VI	·37	·25	·22	·58	·81	—	·48	·41
Substitution ...	VII	·30	·13	·27	·45	·53	·48	—	·42
Maze (Porteous) ...	VIII	·26	·14	·32	·44	·47	·41	·42	—

Table V. *Inter-correlations between tests* (37 *boys*).

Test	No.	I	II	III	IV	V	VI	IX
Inspecting	I	—	·33	·35	·19	·33	·28	·42
Machine-minding ...	II	·33	—	·03	·26	·08	·14	·21
Pairing	III	·35	·03	—	·17	·30	·14	·05
Memory drawing ...	IV	·19	·26	·17	—	·46	·41	·32
Intelligence (Binet)	V	·33	·08	·30	·46	—	·81	·32
Intelligence (Group)	VI	·28	·14	·14	·41	·81	—	·19
Form Relations ...	IX	·42	·21	·05	·32	·32	·19	—

Table VI. *Inter-correlations between tests* (41 *girls*).

Test	No.	I	II	III	IV	V	VI	IX
Inspecting	I	—	·25	·42	·43	·24	·46	·14
Machine-minding ...	II	·25	—	·42	·32	·26	·25	·37
Pairing	III	·42	·42	—	·32	·19	·21	·12
Memory drawing ...	IV	·43	·32	·32	—	·72	·63	·53
Intelligence (Binet)	V	·24	·26	·19	·72	—	·86	·47
Intelligence (Group)	VI	·46	·25	·21	·63	·86	—	·69
Form Relations ...	IX	·14	·37	·12	·53	·47	·69	—

From these tables it would appear that the relations between the various tests are as follows:

(1) Test I correlates best with intelligence for girls, and with form relations for boys.

(2) Test II correlates best with Test III for girls and Test I for boys, but in both cases correlation with immediate visual memory as measured by Test IV is significant. This is not surprising when the processes in the two tests are compared.

(3) Test III correlates best with Test I for both girls and boys. This suggests that the inspection of given forms has a special characteristic of its own.

(4) The average correlations with Tests V, VI, VII and VIII for the combined group is:

Test I, 0·335; Test II, 0·19; Test III, 0·27.

The average correlation with Tests IV, V, VI, VII and VIII for the combined group is:

Test I, 0·33; Test II, 0·22; Test III, 0·26.

If these additional tests be regarded as measures of intelligence (separately or combined) the correlation of Tests I, II and III with intelligence is not high.

Other methods of examining these tables of coefficients are, however, available. We may apply the partial correlation formulae of Yule to see if any significant relationships remain when the factors measured by one kind of test are considered as without influence on the results of other tests.

In Table VII the partial correlations obtained from the application of Yule's formulae to selected coefficients of Table IV are given. The variables selected are: (1) Memory drawing (Test IV); (2) Intelligence (Test VI); (3) Inspecting (Test I); (4) Machine-minding (Test II). Thus the coefficient $r_{34\cdot 12}$ (0·058) signifies the extent of the relationship between 'inspecting' and 'machine minding' when the effects of 'intelligence' and 'immediate visual memory' are held constant.

Table VII.

$r_{12}=\cdot 58$	$r_{12\cdot 3}=\cdot 56$ $r_{12\cdot 4}=\cdot 54$	$r_{12\cdot 34}=\ \cdot 525$	Correlation (due to '*g*' ?) unaltered by the two 'attention tests'
$r_{13}=\cdot 37$	$r_{13\cdot 2}=\cdot 25$ $r_{13\cdot 4}=\cdot 31$	$r_{13\cdot 24}=\ \cdot 248$	
$r_{14}=\cdot 25$	$r_{14\cdot 2}=\cdot 023$ $r_{14\cdot 3}=\cdot 26$	$r_{14\cdot 23}=-\cdot 026$	
$r_{23}=\cdot 31$	$r_{23\cdot 1}=\cdot 234$ $r_{23\cdot 4}=\cdot 127$	$r_{23\cdot 14}=\ \cdot 081$	
$r_{24}=\cdot 35$	$r_{24\cdot 1}=\cdot 132$ $r_{24\cdot 3}=\cdot 286$	$r_{24\cdot 13}=-\cdot 16$	Correlation between 'machine-minding' and 'intelligence' (due to '*g*' ?) removed when visual memory test (which possesses a high '*g*' factor) is held constant.
$r_{34}=\cdot 29$	$r_{34\cdot 1}=\cdot 224$ $r_{34\cdot 2}=\cdot 205$	$r_{34\cdot 12}=\ \cdot 058$	Correlation between 'inspecting' and 'machine-minding,' reduced when effect of 'intelligence' is held constant

Table VIII. *Partial correlations for the four variables.*

(1) Intelligence (Stanford Binet); (2) Inspecting; (3) Machine-minding; (4) Pairing.

$r_{12}=\cdot 41$	$r_{12\cdot 3}=\cdot 36$ $r_{12\cdot 4}=\cdot 34$	$r_{12\cdot 34}=\cdot 33$
$r_{13}=\cdot 26$	$r_{13\cdot 2}=\cdot 16$ $r_{13\cdot 4}=\cdot 21$	$r_{13\cdot 24}=\cdot 13$
$r_{14}=\cdot 27$	$r_{14\cdot 2}=\cdot 12$ $r_{14\cdot 3}=\cdot 22$	$r_{14\cdot 23}=\cdot 08$
$r_{23}=\cdot 29$	$r_{23\cdot 1}=\cdot 22$ $r_{23\cdot 4}=\cdot 22$	$r_{23\cdot 14}=\cdot 14$
$r_{24}=\cdot 41$	$r_{24\cdot 1}=\cdot 34$ $r_{24\cdot 3}=\cdot 32$	$r_{24\cdot 13}=\cdot 13$
$r_{34}=\cdot 24$	$r_{34\cdot 1}=\cdot 18$ $r_{34\cdot 2}=\cdot 19$	$r_{34\cdot 12}=\cdot 09$

Study of Table VII suggests that the correlation between Tests I and II and the others (as well as between themselves) is due to a factor which is found to a high degree in the Intelligence Group test and in the Immediate Visual Memory tests. In Table VIII we have a similar result.

The same method may be applied to the remainder of the variables in Table IV but for our immediate purposes it is unnecessary.

In some respects a more convenient method of analysis is provided by the use of Spearman's tetrad-difference criterion.

If the correlations in Table IV be examined no very large tetrad differences are obtainable. The following are some of the largest:

Table IX.

	Intelligence (Group)	Machine-minding	
Stanford Binet	·81	·26	$F = \cdot81 \times \cdot35 - \cdot58 \times \cdot26$ $= \cdot2835 - \cdot1451$
Memory drawing	·58	·35	$= \cdot1384$
	Memory drawing	Machine-minding	
Stanford Binet	·61	·26	$F = \cdot81 \times \cdot29 - \cdot31 \times \cdot26$ $= \cdot1769 - \cdot0806$
Memory drawing	·31	·29	$= \cdot0963$
	Intelligence (Group)	Machine-minding	
Stanford Binet	·81	·26	$F = \cdot81 \times \cdot29 - \cdot37 \times \cdot26$ $= \cdot2349 - \cdot0962$
Inspecting	·37	·29	$= \cdot1387$

The P.E. of $F = 0{\cdot}037$[1] (five times the P.E. of $F = 0{\cdot}185$). There are, therefore, no tetrad differences greater than five times the P.E.

Hence, according to Spearman's theorem, the correlations between these eight tests are entirely due to the operation of a common factor '*g*,' and the correlation between each test and '*g*' may be calculated. These correlations[2] are as follows:

Table X.

	Correlation with '*g*'
Intelligence (Stanford Binet)	·83
Intelligence (Group) ...	·76
Memory drawing	·71
Substitution	·61
Mazes (Porteous)	·57
Inspecting	·55
Pairing geometrical forms ...	·44
Machine-minding	·37

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[1] Formula in appendix, Spearman's *Abilities of Man*.

[2] Calculated from the formula $r_{ag} = \sqrt{\frac{A^2 - A'}{T - 2A}}$.

It is apparent that neither Test I nor Test II is as good a measure of '*g*' as the so-called 'intelligence' tests, but that to a certain extent '*g*' enters into success in each. There are, however, large specific factors to be considered. According to Spearman, since the tetrad difference criterion is satisfied, none of these specific factors should cause any significant amount of correlation when the influence of '*g*' is removed.

Using Yule's formula for partial correlations the inter-correlations of the eight tests for '*g*' constant may be calculated. They are given in Table XI.

Table XI. *Inter-correlations for* 'g' *constant* (*derived from Table IV*).

(Arranged in order of Table X.)

Test	No.	V	VI	IV	VII	VIII	I	III	II
Intelligence (Binet)	V	—	·49	·05	·07	−·03	−·09	−·19	−·09
Intelligence (Group)	VI	·49	—	·04	·04	−·05	−·08	−·20	−·05
Memory drawing ...	IV	·05	·04	—	·04	·06	−·13	−·15	·13
Substitution ...	VII	·07	·04	·04	—	·11	−·05	−·10	−·13
Mazes (Porteous) ...	VIII	−·03	−·05	·06	·11	—	−·08	·09	−·09
Inspecting	I	−·09	−·09	−·13	−·05	−·08	—	·23	·11
Pairing	III	−·19	−·20	−·15	·01	·09	·23	—	·09
Machine-minding ...	II	−·09	−·05	·13	−·13	−·09	·11	−·09	—

There are only two coefficients in this table that are large enough to require comment. Theoretically all of them should be of zero value, so that the question arises whether any of them are too large to be explained by the unavoidable errors of measurement.

The two exceptional coefficients are those between the two Intelligence Tests V and VI (0·49) and between Test I and Test III (0·23). In both these cases there is an important resemblance between the kind of material employed and the mental processes which take place. Inasmuch as 'overlap' may occur between tests which resemble one another[1], we seem to have here two examples of this phenomenon.

Similar results have been obtained for a different correlation table made up of eight variables, the test of Form Relations (Test IX) being, in this case, included in place of the Substitution test. The distribution of tetrad differences is now as in Fig. 5.

It will be observed that the tetrad difference criterion is once again satisfied. Moreover a specific correlation (0·46) remains between the Stanford Binet test and the Intelligence Group tests (and also between Test I and Test III) when the effect of '*g*' is held constant, but in this case there also appears to be an 'overlap' between the Memory Drawing test and the Form Relations test (represented by a coefficient of 0·22).

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[1] Cf. Spearman, *op. cit.*

We may conclude, therefore, that although the correlations in Table IV are largely due to the influence of a common factor '*g*,' there is some similarity between Tests I and III which produces an additional correlation between them. For the rest there are no other connections. The results of each test are influenced by large specific factors, and we may regard success in Test II as being mainly due to such specific factors, the effects of '*g*' being in this case small.

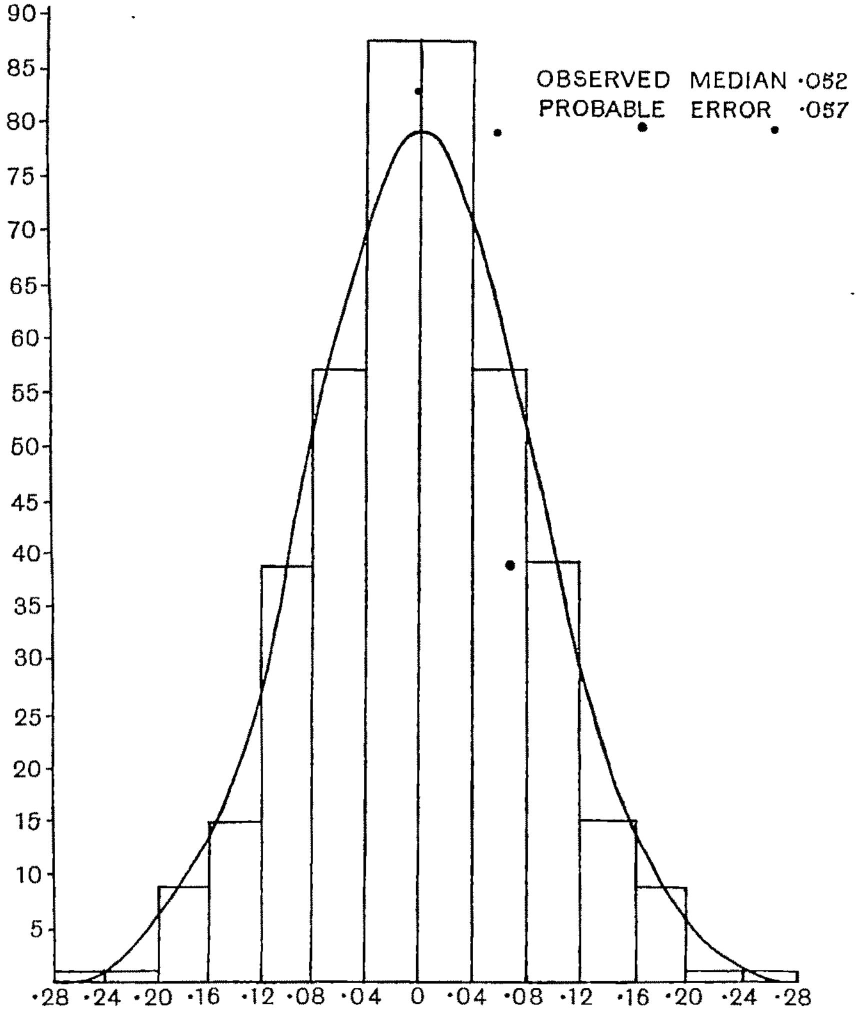

Fig. 5. Observed and theoretical distribution of tetrad differences.

IV. CRITICAL REVIEW AND CONCLUSIONS.

It is clear from the results of the experiments described in the last section, as well as from the work of previous investigators, that the problem of measuring differences in the ability of persons to 'pay attention' is a difficult one. It is, apparently, impossible to avoid the complications of other factors, and although we may reduce the effects of experience or of acquired manual dexterities, as we have done in these enquiries, we cannot remove the influence of general intelligence or whatever that general ability may be, which finds expression in all mental activities.

It has been suggested that the so-called general intelligence is in fact the ability to 'pay attention.' But it has been called other things too, such as 'creative ability' and 'reasoning power,' and while it may be true that the quality of attention is a factor in intelligence behaviour, it hardly seems likely that intelligence and the quality of attention are identical. McQueen[1] in his researches concluded that there was no 'power,' either to distribute attention or to concentrate attention, of general application and capable of measurement, and he was inclined to regard the connections which he found between dissimilar tests as the effects of the general factor '*g*.'

Spearman[2] has endorsed this view and has declared that attention in its different aspects merely represents 'alternative dimensions of the same cognitive output characterizing each individual.' This is as much as to say that there is no ability 'to attend to several things at once' which is distinctly different from the ability 'to concentrate attention upon a single object.' The capacity for success in activities dependent upon these is due, according to Spearman, to a general ability which expresses itself in either way, while any abilities which are called out by one task rather than another are regarded as special and peculiar to that particular task. Consequently there can be no 'group' factors of concentration or of diffusion of attention (additional to the general ability above mentioned), which may be shown in several dissimilar activities.

But in all the tests mentioned by Spearman[3] in his argument on this particular subject, attention, as given, appears to consist of a succession of concentrations, the diffusion of which he speaks being chiefly shown in the continuity of purpose. Moreover the processes required in those tests are of a highly intellectual order, and possibly are not suitable for comparison with those in which attention may be said to be given at lower perceptual levels. Many of the processes of everyday life which are spoken of as requiring a distribution of attention are of the latter kind. Mental and manual processes often go together. If we consider the 'non-mental' aspects of manual dexterity, and the fact that tests of dexterity usually correlate very slightly with tests of intelligence, it is difficult to see how the conditions of attention in these are related to the conditions of attention in more intellectual processes, simply through the operation of a general factor which chiefly appears in the latter.

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[1] McQueen, *op. cit.*

[2] Spearman, *op. cit.*

[3] Spearman, *op. cit.*

Be that as it may, there can be no doubt as to the generality of some factor underlying success in all the tests described in this report. The results obtained are all consistent with the belief that success in these activities is dependent, in part at least, upon a general factor (Spearman's '*g*'). But the correlation between the tests specially introduced as measures of 'attention' and those in which the '*g*' factor is apparently dominant is not as high as might be expected if success in them were *largely* due to the influence of this factor. Hence we can only conclude that in each of these two tests specific factors are operative to a considerable degree. (By 'specific' is meant peculiar to each test or process.) Apart from a suggestion of 'overlap' between Tests I and III (and of course between the two verbal intelligence tests), there is no suggestion of any relationship between Tests I and II or between any other pair, other than that due, on Spearman's theory, to the general factor '*g*.'

But these results do not necessarily mean that the factor of attention is the *same* as the '*g*' factor. It is certain that the intensity and duration of the attention required for success (other things being equal) vary from one task to another. We may, therefore, regard the peculiar combination of intensities and durations of attention which are called for by a particular task as one of the components of the specific factors by which success in the task is gained. In other words, we may have to seek for the solution of the problems of distributed attention not in the measurement of an individual's average endowment of '*g*,' but rather in the determination of his abilities to adjust himself satisfactorily to the special circumstances of the task. But there are also other special factors operative which must be taken into account. For instance, as already mentioned, the speed and accuracy of perception is frequently influenced by the factor of 'preparedness'[1]. However, it is the task of the experimental enquirer to allow for or to eliminate the influence of factors such as these.

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[1] Cf. experiments of C. Fox (*Educational Psychology*, Kegan Paul, 1926). It is also related of a famous professor of botany that seeking a certain species of plant he went directly to a spot where, from consideration of its nature, he expected to find it growing. He found it there, but on his return, with its appearance now more clearly in his mind, he

From the point of view of the vocational psychologist, the important thing in all practical enquiries is to determine as clearly as possible what are the essential characteristics of any given activity. Among these are the conditions under which attention is to be given. Let us examine these conditions, choosing as an illustration such a task as that of cancelling O's in a printed page. The examinee is instructed what to do, and, assuming that he has sufficient general ability to grasp these instructions, he begins to search in a systematic way for the O's. There then ensues a succession of conscious stages, a succession of attentions, in which letters are recognized as 'O' (and the action of crossing it out is then initiated) or as 'not O' (when the action is inhibited). It is not necessary that all these successive attentions should be of equal intensity; the O's certainly receive attention at higher intensity than the 'not O's,' for the perusal of each line is arrested at every O that is identified. The lower the intensity of the attention to the non-significant letters, the more quickly can the lines be examined, and the number of O's crossed out can be correspondingly increased. But to maintain a high intensity of attention for one particular stimulus and to deal with all other stimuli at or near the 'margin' of consciousness is to 'concentrate' attention, in the generally accepted meaning of the term. Such concentration enters into the recall of synonyms, the completion of sentences, the classification of ideas and the like. All such mental processes appear to be linked by the general factor '*g*,' and to give quite high measures of it. It might seem, therefore, as if 'concentration' were an element within '*g*' if it be not '*g*' itself. How then does 'distribution' enter?

It has already been remarked that although Test I requires a high intensity of attention for each of the three components of the diagram, there is a ***distribution*** of attention in that the cognition of each component takes place separately and in succession. The normal method of distributing attention being by alternation or succession (cf. McQueen's work), it is clear that every activity which requires more than one single cognition introduces a distribution of attention. In the process of cancelling 'O's' described above, the 'natural diffusion of attention' of which Spearman speaks consists in keeping in mind the nature of the task so that the proper succession of attentions to 'O's' and 'not O's' is maintained.

saw it in many other places, until, approaching his own home, he found it growing near, and visible from, his own front door.

Looked at in this way, there are no differences in the *nature* of attention as between its concentration and its diffusion[1]; but there are differences in the *rate* of the succession and in the *intensity* of attention necessary for the purposes of the moment. Illustration is provided by the procedure of Test II. In this test the situation requires clearness of cognition or intensity of attention at crucial moments, just as in the cancellation of letters and as in Test I; but although the time intervals are fairly long, several events may occur in rapid succession, and attention must be adapted accordingly. Consequently a state of 'balance' tends to be set up in which attention to each important stimulus is given at such intensity that the sequence can be successfully maintained. It would appear that a tendency to concentration upon any one stimulus prevents such an equilibrium; hence a person who habitually thus concentrates may fail. Nor does it seem difficult to produce a similar condition of affairs in the cancellation test. If the 'O's' were to be crossed out, not in the simple sequence of their occurrence, but according to some given rule (*e.g.* cross one miss two, cross two miss one, and so on), the situation would be not unlike that above described. The person performing the task must then not allow the intensity of his attention to any portion of the whole task to interfere with the smooth performance of the remainder. Even though he carries through the complete task by a succession of attentions to parts of it (as the majority of persons will do), it is probable that a too high intensity of attention to any single part will interfere with the most successful attention to the remainder.

Hence the differences we have observed between one task and another ought perhaps to be regarded as due to differences in the *speed* as well as in the *intensity* at which successive attentions have to be given, and although the ability to give attention at the requisite *intensity* may be related to the '*g*' factor, the ability to maintain the *speed* may not. As to the succession itself, there can be no question: the so-called simultaneous performance of two or more tasks only occurs when automatisms functioning in different parts of the body are employed. (Thus a series of movements of hand and arm may accompany a series of movements of the muscles of the larynx and other vocal organs; these movements are automatic so that attention, in the sense in which we have considered it, does not enter.)

If this view be accepted, then the differences between individuals may be ascribed to the fact that they are not equally able to give attention successively at the required intensity and at the required speed. Since both these vary from task to task, there may be many different combinations of speed and intensity; there are also many different kinds of material whose intrinsic appeal differs from person to person; there are also various effects of training, practice and special knowledge; so that it is not surprising that the performance of an individual varies from task to task.

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[1] This is what Spearman means when he says that concentration and diffusion are alternative dimensions of the same cognitive output. Spearman, *Abilities of Man*, p. 269.

But it must be admitted that the tasks which have been used for such comparative purposes have differed very greatly from each other. Even Tests I and II differ considerably, although our analysis has shown that in both of them there is a succession of attentions at varying intensities. But in Test I *the rate of succession is within the individual's control*; in Test II *it is determined from without*; and unless the individual can adapt himself to the rate of succession of the stimuli he will fail. Clearly such differences are important, and, as we have seen, the correlation between these two tests is small. Yet, small though it is, it is significant and is closely related to the correlation between these tests and those of general intelligence and immediate visual memory. Even though, on Spearman's theory, these connections are to be ascribed to '*g*,' we might look upon them as representing the extent to which the ability to give attention at the requisite intensity enters into success in all of them. Consequently the fact that the correlations are not higher must be attributed to the large differences in the other conditions of the tests, particularly as regards the rate at which successive attentions are required.

In the Intelligence Group test the rate of succession depends upon the individual himself, and similar conditions prevail in cancellation, Stanford Binet (in the separate tests), Form Relations Group test, and the like. Probably the large differences in the nature of the materials employed in these tests partly account for the comparatively low correlations of these tests with each other. But as regards Test II and similar tasks in which the rate of successive attentions is outside the individual's control, this itself constitutes an important specific factor. In tapping and adding, in card sorting, and counting by threes, and in the other simultaneous disparate activities investigated by McQueen, the rate of succession of attention is very high. To some extent it is within the individual's control. But the setting up of a satisfactory 'balance' or 'equilibrium' (above mentioned) is hindered by the need for making the muscular adjustments work automatically. Consequently no large connections between the various activities investigated were discovered. It may be, therefore, that an extremely high speed of successions of attention requires a specific set of conditions which vary according to the nature of the tasks to be performed. This would explain the experimental results so far obtained.

V. SUMMARY.

To sum up, the experimental investigation of tasks which are designed to throw light upon differences in the abilities of individuals 'to pay attention' points to:

1. Resemblances (not large, however) between tasks which require for successful performance a high intensity of attention and in which the succession of attentions (if required) is within the individual's own control. Such resemblances seem to correspond to the influence of a '*g*' factor; they are also most noticeable in tasks which require a relatively high intensity of attention (or concentration).

2. Marked differences between tasks which require for successful performances (i) a succession of attentions at a very rapid rate, or (ii) a succession of attentions at a rate which is outside the control of the individual. Such tasks may be connected by the fact that a certain intensity of attention is also needed, but the resemblances are slight. Most of the 'distributed' attentions are of this kind.

3. The conclusion that specific factors are large in distributed attentions, because:

(*a*) the task calls for some special adaptation based upon knowledge, acquired skill, or special sensory and muscular equipment;

(*b*) individuals differ in sensory acuity and muscular adaptability;

(*c*) the 'natural rate' of the individual is different from that required for the task, some readjustment being necessary;

(*d*) the rate of change of attention differs from task to task.

But it is necessary to note that the majority of the tasks which have been used in experimental work on these problems belong to the class in which successive attentions, if required, are given at the individual's own rate. It seems advisable to examine other instances in which the more specialized conditions of 'distribution' obtain. There are many of these in everyday life; the practical psychologist must, therefore, devote more time to their study. It may be that tasks of the second type are to be found so closely resembling one another in the processes of attention which they require that correlations may be obtained much larger than any contained in this report.

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TESTS OF APHASIA.

BY CHARLES FOX.

I. *Do certain tests of aphasia involve the use of language* (pp. 242–243).
II. *A method of testing the tests described* (pp. 243–245).
III. *Results obtained by this method* (pp. 246–250).
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V. *Does aphasia imply a diminution of intelligence* (pp. 253–255).

I. DO CERTAIN TESTS OF APHASIA INVOLVE THE USE OF LANGUAGE.

IN his classical work on aphasia Dr Henry Head made use of a series of hand, eye and ear tests. The patient sat facing the physician who touched his own right or left eye or ear with his right or left hand; and the patient was instructed to reproduce the movements. Or alternatively, the outlines of such movements were drawn on cards which were placed in front of the patient with the instruction to imitate them. As the patients had to reverse the seen movement the test was difficult for them and mistakes were frequent. Amongst the variety of tests for aphasia which Dr Head employed, he makes the remark that this is the only one in which a perfectly normal person may make blunders. Anybody who has watched a squad trying to imitate the movements of their instructor will be able to confirm the fact that normal people by no means find this sort of test easy.

Now what precisely does such a test involve and is it a test for aphasia, *i.e.* does it imply the use of language in any form? Dr Head is convinced that it does. In his book on *Aphasia* (Pt II, ch. 2, p. 170) he says that when the card is in front of a person "the words 'right' and 'left,' 'eye' and 'ear' must be silently interposed between the reception and the execution of the order." Again, on p. 198, the following statement is made: "When a patient, sitting opposite me, attempts to imitate movements of my right or left hand brought into contact with an eye or ear, some degree of internal verbalisation occurs as a phase of the normal act. No sound need be uttered; but the words 'right' and 'left,' 'eye' and 'ear' or some more inclusive expression such as 'opposite' or 'the same' are essential to correct imitation of this kind....For the same reason patients may experience considerable difficulty in carrying out these tests, when the order is placed before them in the form of a picture."

If this view is correct the test is an excellent instrument of investigation for detecting language difficulties in aphasics. In the employment of any psychological test there is always a danger to be avoided known as the 'psychologist's fallacy[1].' This arises from the fact that the experimenter is apt to suppose that the subject will respond to a stimulus or an order in the same way as he himself would respond in like circumstances. The only way of avoiding this difficulty is to secure careful introspection from the subject as soon as the task is completed. Wherever possible, Dr Head took full advantage of this with his war patients. However, aphasics are not the best people to perform the difficult task of introspection satisfactorily, and it is safer to get introspective data from normal people.

The question arises as to whether the tests really do involve the use of language or whether inability to perform them correctly rests on some other defect. It occurred to the writer that the best way of solving the problem was to try the tests on perfectly normal persons and to secure introspective evidence by this means. In this way it was hoped not only to evaluate the tests from the point of view of language, but to discover what defect was present in patients who failed to perform the tests accurately.

II. A METHOD OF TESTING THE TESTS DESCRIBED.

In order to carry out a psychological investigation of this nature it is necessary to adopt a fairly rigid technique and to adhere to it with every individual subject. It is also essential to take great care to avoid suggesting to the subject what one is looking for. The subjects were University honours graduates and certain schoolboys. In dealing with the graduates the experimenter said: "I am going to show you some pictures which suggest movements and I want you to perform these movements." To the schoolboys the instruction was: "I am going to show you some pictures and I want you to do just what the man is doing in the picture. Do you understand?" Head's diagrams drawn to the size of a postcard were then placed, one by one, on a table before the subject, who sat facing the experimenter. The card remained on the table until the subject had performed the movement, when it was withdrawn and immediately afterwards the next one was exhibited; and so on through the series. The order of presentation was the same in every case, and nothing was said until the whole series was completed. The series is shown here for reference (reduced in size) in the order employed. When the whole eight had been shown they were repeated in the same order; making sixteen tests in all. As soon as the series was completed the following questions were asked and the replies recorded verbatim: (1) "Was it easy?" (2) "Tell me how you did it?" In case the answer to question (2) proved unsatisfactory in getting a full introspective account, the question (3) was asked: "What did you think of before you did any one?"

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[1] See C. Fox, *Practical Psychology*, Introduction, p. xv. Kegan Paul, 1928.

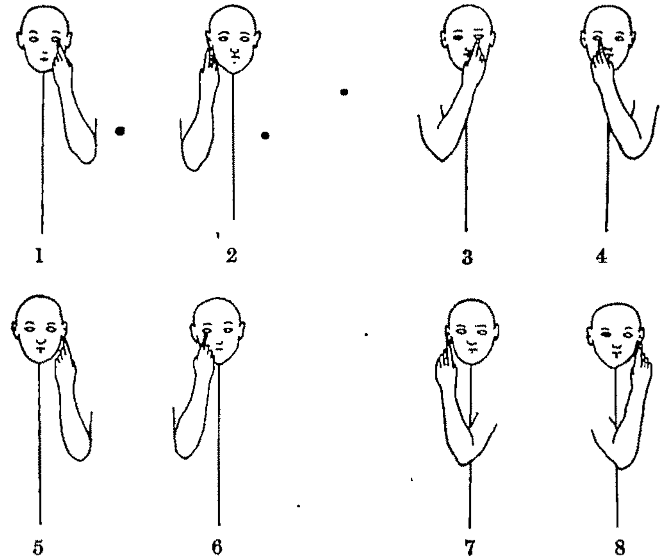

Diagram I.

Certain of the schoolboys did the whole series incorrectly, reversing every movement, *i.e.* they ignored the fact that the picture as they see it must be turned round in imagination before they can perform the movements. Such cases are purely imitative and will be referred to as 'automatic'; and for these the following questions were asked after the task was completed. (1) "Did you do them right?" If the subject asserts that he did them correctly, he is shown Diagram I, 1 again and told to repeat the movement. Then, if he is still incorrect, he is asked (2), "Are you doing just what the man is doing?" If he persists in saying that he is, the final question (3) is asked, "Which arm is he moving?" By this means it was possible to discover in practically all cases what method was employed. More especially it was possible to test Head's view that language is an essential part of the process.

Now if we consider the problem that the subject has to solve we realize that there are three possible ways of doing it. He may give himself a verbal order by the use of 'internal speech,' such as "I must do the opposite," or "I have to use the opposite hand," or "The left hand on the picture corresponds to my right"; and this was the method used by some of the graduate subjects. But the problem may be tackled by spatial manipulation only, without the use of internal speech; and this may take various forms. The person may imagine that the diagram is turned round, and consider himself as looking through the card from the back as though it were transparent. Or he may imagine the card placed behind him so as to face the same way as he himself is looking. It would usually be said that this involves the use of visual imagery, but if any imagery is employed it is so vague as hardly to be recognizable. The ability to think of objects as turned round is acquired so early in life that, if images were originally used, they have long since disappeared from consciousness. For images tend to disappear as soon as they have accomplished their purpose of making thinking facile; and when thought flows freely images play no part in the process[1]. We may describe this procedure of spatially manipulating the card in imagination as 'orientation of the diagram.' Finally, it is possible to solve the problem by means of orientation of the body, and this may be accomplished in a variety of ways which it is difficult to separate. The more usual method involves the use of kinaesthetic imagery or incipient bodily movements. The person imagines his own body taking up the position represented on the picture. It is very doubtful whether pure kinaesthetic imagery has any independent existence, or whether in all such cases incipient bodily movements are alone involved. The spatial schema of our body which we all carry about with us in consciousness is more or less easily manipulated. Some subjects aver that they 'feel' their bodies turned round in space so as to be in the position of the picture. Others 'feel' nothing, but simply 'know' that their bodies have made the necessary turning movement. Careful observation of the subjects discloses the fact that incipient movements of turning the body are made; and in some cases the movement is very pronounced, so as to constitute a quarter or half a turn of the body in the chair. This is more especially the case with children, and I have observed some schoolboys who actually got up and turned completely round in their effort to solve the problem. The orientation sometimes takes a visual form, and the subject 'sees' himself turned so as to occupy the position of the man in the sketch. For purposes of discussion it is convenient to refer to all the cases of this last group, namely kinaesthetic, incipient or actual movements, or visual realization of movement, as 'orientation of the body.' The above analysis is based on the actual procedure adopted by the persons who were examined.

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[1] C. Fox, "Conditions which arouse images in thought," *Brit. J. of Psychol.* VI, General Section.

III. RESULTS OBTAINED BY THIS METHOD.

We shall next consider the different groups of subjects on an age basis. There were twenty-one honours graduates, all of whom were members of a practical class in psychology and had, therefore, some experience of introspective methods. It was clear from the introspections, obtained by dictation from each person immediately after he was tested, that some of the subjects used a mixed method; and probably closer investigation would show that this is more common. As the same person may use a variety of methods, the numbers in the different categories named below will not correspond to the number of persons tested. Twelve subjects oriented the body, eleven used a verbal method (internal speech) and five oriented the diagram. Only four of them succeeded in doing the whole series without an error. The errors were distributed as shown:

Errors ...	0,	1,	2,	3,	4,	5
No. of subjects	4,	5,	7,	1,	2,	2 = 21.

In some cases the mistakes were corrected immediately they were made, so that out of a total of 40 errors 10 were corrected spontaneously. In some other cases the mistakes were realized but not corrected.

The next group of subjects consisted of forty boys from a selective Central School, *i.e.* a school which selects its pupils by competitive examination from elementary schools. Their ages ranged from about 12 to 15 years. In this school the forms are classified as bright, medium and dull by the headmaster. There are four bright classes and four dull; and the five cleverest boys from each of the bright classes, and the five stupidest from the dull classes acted as subjects. Introspection records were made for each boy in the manner indicated above, and the analysis here given is that for the brightest boys. There were eleven out of the twenty who oriented the body, ten employed words (internal speech) and three oriented the diagram. This is very similar to the distribution found amongst the graduates. But with these subjects a new phenomenon presents itself; for there were three who were completely automatic. Only one succeeded in doing the whole series without an error. The errors were distributed thus:

Errors ...	0,	1,	2,	3,	6,	7,	10,	16
No. of subjects	1,	2,	5,	6,	1,	1,	1,	3 = 20.

Excluding the automatic cases, there were 53 errors of which 13 were spontaneously corrected.

The results obtained from the twenty dullest boys in the school show a different state of affairs. The striking feature of this group is the large amount of mimicry, for nine of the twenty were purely automatic and another was practically so. There were five who oriented the body, six were verbal (internal speech) and three oriented the diagram. The errors were distributed thus:

Errors ...	0,	1,	2,	3,	4,	5,	7,	13,	14,	16	
No. of subjects	0,	2,	1,	2,	2,	1,	1,	1,	1,	9	= 20.

Of the 57 errors made by the eleven non-automatic subjects 13 were corrected spontaneously. There were indications that the automatic group were easily suggestible.

In order to see what would be the result of defects of speech on the ability to do these tests all the boys who stammered were tested. Only nine cases were found in the school, eight of whom stammered only when reading or excited, and one stammered badly. Four out of the nine were completely automatic. Of the rest three oriented the body, two the diagram and there were none who used internal speech. The number of cases is too small to draw any certain conclusion, but, as far as they go, they tend to show that a difficulty in the use of speech makes this group of stammerers approximate in its performance to the group of dull boys. Speech deficiency has the same effect as dullness.

It is interesting and valuable to discover what is the lowest age at which the problem presented by these tests is capable of solution. For this purpose thirty-three boys from a good elementary school were examined. They were carefully selected by the headmaster, twenty-three being the brightest for their age in the school, and ten the dullest.

The dull boys had an age range from 11 years to 9 years. They all understood the problem and carried it out without hesitation. Many of them were very prompt, doing it immediately. Nevertheless not a single boy did any test correctly. It appears, therefore, that for dull pupils the problem is too difficult for accomplishment at 11 years, but we saw above that it can be accomplished by them at the age of 12. Six of the subjects slavishly copied the diagram and consequently reversed every movement; they were purely automatic. In the other four a similar state of affairs was evident and they, in addition, failed to notice in three or four of the tests that the hand should have been crossed over the body.

With these boys Diagram I, 1 was shown again at the end of the series, and they were asked to repeat the movement. Each did it incorrectly again. Then, whilst his hand was in the incorrect position, he was asked whether he was sure that what he was doing was correct. Three of the boys realized that the movement they had made was wrong. Six of the others insisted that the position was correct, and only realized their error when they were asked to state in words which hand the man in the diagram was using and which they were using. The remaining subject declared that the diagram displayed the right hand. I repeated the movement myself, and he still maintained that the right hand was used. When I turned my back on him he said it was the left hand, but on turning again to face him he declared it was the right again. It is obvious that this boy only thinks of right and left with reference to himself, and has not yet learnt to think of these positions with regard to some other object. In this he clearly shows what defect some aphasics suffer from, namely, an inability to conceive spatial relations correctly.

The twenty-three bright boys may be separated into two groups, (*a*) and (*b*), according to age. The first group consisted of ten boys whose ages ranged from 11 years to 9 years and the remaining thirteen had an age range from 8 years 5 months to 7 years 9 months.

In the former group there were three who oriented the body, four who imagined the diagram turned round, one of whom also used internal speech. The remaining three were automatic, but two of them realized their error when they were shown Diagram I, 1 again, and were asked to put the problem into words. None of them corrected his errors spontaneously. The errors were distributed thus:

Errors ...	0,	1,	2,	3,	4,	8,	16
No. of subjects	0,	1,	1,	2,	2,	1,	3 = 10.

The only thing which differentiates this group of younger bright boys from the previous group of older bright boys is the inability to correct the errors spontaneously. In other respects they are similar.

We may say, then, that the ability of clever boys to perform this series of operations does not vary appreciably between the ages of 9 to 15 years. And, if we exclude the cases of pure imitation, there is little difference between the youngest boys and the University honours graduates. The spatial ideas involved in performing this sort of problem and the formulation of it in internal speech, where this is employed, are abilities which are fully developed in clever boys at the age of 9.

When we turn to the younger group of bright boys we meet with a different situation. All of them turned out to be purely automatic, for in every one of the thirteen cases there were 16 errors, and practically all the movements were reversed from right to left or *vice versa*. Seven of the subjects failed to cross the central line of the body in two or three instances where the tests demanded it. Each boy, as before, was shown Diagram I, 1 again at the end of the series, and when he had again performed the wrong action he was asked to state in words what the man was doing. Seven of them realized their error, and corrected it when they were thus forced to formulate the problem in words. In other instances this could not be done, and the errors persisted.

Three cases are instructive and may be quoted more fully. A boy of 8 years used his right hand instead of the left, and insisted that the man was doing so. On being pressed he changed his ground and said the man was using his left hand. Then he said: "I forget which is right and which is left." When he was asked to show his right hand he showed his left and *vice versa*. Another boy of 8 years maintained firmly that the diagram's left hand was the right. When I repeated the movement sitting opposite to him he also said I was using my right hand. He could only be persuaded to the contrary by standing behind my back whilst I was performing the movement. A boy of 7 years 9 months who had made 16 errors, which included six cases where he had failed to cross the central line of the body, was asked to show his left hand and his right eye, which he did perfectly well. He was then shown Diagram I, 1 again and did it wrong, as before. He used his right hand, and insisted that the diagram was doing the same. Nevertheless, although he could point quite easily to my right or left hand, he invariably confused these when shown a diagram which was in the same position with regard to him as I was. This case clearly shows that there is a greater difficulty in symbolic formulation when interpreting a simple diagram than when interpreting the movements of a person placed opposite to oneself.

We may conclude that the spatial ideas underlying the sort of problem involved in Head's tests, and the ability to formulate symbolically the meaning of a simple abstract diagram, do not become habitual with clever boys until the age of 9 years and with dull boys until the age of 12 years[1].

For comparative purposes I have analysed the clinical records of Head's war patients given in *Aphasia*, vol. II. There were sixteen of such cases examined by the present set of tests, but unfortunately only thirteen had the whole series of sixteen cards. The errors were distributed thus:

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[1] I was unaware until this paper was finished that Head had anticipated this result. Thus he says, *Aphasia*, I, 396, "Asked to imitate the movements of the examiner, when face to face, he may give up all attempt to employ the right or left hand correctly and consistently select the one that is opposite. Power to carry out this test accurately is acquired between the ages of nine and twelve, and the aphasic reverts to the simpler method of response habitual in childhood." Head gives no indication as to where he got his figures.

Errors ...	0,	1,	2,	3,	4,	5,	6,	11,	12,	13,	15,	16
No. of subjects	0,	1,	1,	1,	1,	2,	1,	2,	1,	1,	1,	1 = 13.

We may safely assume that the patient with 15 errors was completely automatic, so that this group contains two automatic cases. Excluding these, there were 73 errors of which the records show that 5 were spontaneously corrected. The distinguishing feature of the group compared with the graduates and the older schoolboys is the greater inability to correct errors spontaneously. It must be remembered that Head's procedure was far less rigid than mine, and there is nothing to indicate that the instructions given to his patients were invariable. If a rigid method had been adopted, it is probable that the total number of errors would have been greatly increased. Moreover, each of his patients had preliminary practice in the simpler task of imitating the movements of the physician before he was tested with the cards; which would tend greatly to reduce the number of mistakes. Despite this, the mistakes are much greater relatively than in the other groups, and there is a greater proportion of cases with a considerable number of errors. The surprising feature of the group, however, is that the number of automatic cases is very much smaller than we should have expected, and this is probably due to the preliminary practice just mentioned. Whatever particular intellectual defect these patients sustained as the result of their wounds did not reduce their mental level for this task below the age of 9 to 12 years.

IV. LOSS OF LANGUAGE IN APHASIA IS AN INDICATION OF A MORE GENERAL LOSS OF FUNCTION.

We may now attempt to get an answer to the question we started with, namely, whether inability to cope with the tests is due to some defect of speech or to some other deficiency. It has become a commonplace in modern psychology to argue from the abnormal to the normal. Such a method is beset with innumerable pitfalls, and it is debatable whether the procedure is ever really valid. We are on much firmer ground if we travel in the contrary direction and reason from the normal to the pathological. It would be a great benefit if all clinical tests were first of all tried out on normal people, so as to estimate the deviations from normality in pathological cases. This was the method adopted by Alfred

Binet in securing tests for mental deficiency. He standardized his tests by means of normal children, and then tried them on backward and deficient children. That his method was the sound one is shown by the immediate fruitfulness of his work and the astonishing growth in the use of mental tests since his time; all standardized in the same way.

If we turn to our graduates and the older brighter schoolboys, we find that, out of a total of forty-one subjects in these two groups, twenty-one employed internal speech in order to carry out the tests. But of the twenty-one, fourteen also made use of some form of orientation, and the introspections showed that the speech was merely subsidiary to the other method and was not constantly employed. In many cases speech dropped out after the first couple of tests. With the remaining groups speech was much less frequently used. To a limited extent, therefore, Head's view is justified, namely, that speech *may* be a part of the process used in doing the tests. But his contention that speech is essential to the process, which cannot be performed without it, is not borne out by our investigation. Speech is merely an incidental aid used only rarely.

If anybody is accustomed to walk up a street one way, he will usually find considerable difficulty in interpreting a photograph of the street taken from the opposite way; and the same difficulty may be observed in any picture taken from an unaccustomed angle. He who has experienced this difficulty will easily realize the essential nature of the problem presented by these tests. It is a difficulty of orientation or spatial discrimination. And it is worthy of note that some of the other tests of aphasia employed by Head, namely the clock tests and the coin-bowl tests, are fundamentally tests of spatial relationships. Some of Head's war patients suffered from grave affections of orientation as shown by the observation that they could not set out the plan of a familiar room or find their way about in familiar surroundings.

Referring again to the group of forty-one persons just mentioned we find that, whether internal speech was used or not, there were thirty-one cases of orientation in carrying out the tests; either orientation of the body or the diagram or both. So that the hand and eye tests are also to a considerable extent tests of the power of discrimination and manipulation of spatial relations.

I am inclined, therefore, to agree with Dr R. Mourgue[1], who thinks that aphasia is a special case of a more general disturbance of function. According to him it is due to a general weakening of the "fonction de découpage et d'opposition de l'intelligence." By this he means an inability to demarcate or differentiate, which shows itself in a lack of appreciation of spatial relationships. He thinks that the varieties of aphasia are reducible to special disabilities in spatial discrimination, and that language is only disturbed because it too involves such demarcation. It is evident that the hand, eye, and ear test, at the very least, involve some sort of spatial perception. Accordingly Dr Mourgue maintains that, in the majority of cases, no language is necessary, but that those who make mistakes experience a difficulty in perceiving spatial relations. Errors are due to the fact that "le sujet ne peut exécuter la transposition spatiale nécessitée pour l'imitation des mouvements de l'expérimentateur placé devant lui." Our experiments show that for normal subjects this view is well substantiated.

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[1] R. Mourgue, "Disorders of symbolic thinking," *Brit. J. of Psychol.* Medical Section, 1921, I.

Further confirmation has been provided by the work of W. van Woerkom[1] who made a careful study of the restitution of function in one of his aphasic patients. For the first 2 weeks this patient was dumb, but after 1 month he was able to name objects and understand speech and thenceforward improved. His process of re-education was so complete, that, after 6 weeks, a superficial observer could have detected nothing abnormal about him except that he expressed himself with difficulty. Nevertheless a careful examination showed that there was profound loss of function. He could not place a ruler parallel to another at a short distance, but crossed them fumblingly; nor could he imitate the directions of a stick moved in the air. With his eyes bandaged he failed to imitate passive movements impressed on his limbs. He could not place an object correctly on one side or another of a stick even if he was shown what to do. He could point successively to two objects in front of him, but could not indicate their spatial relation either by words or gestures. In fact his geometrical sense was abolished almost completely.

He could carry out purely habitual acts, such as lighting a cigar, since these do not involve any idea of the movement to be made, but are secondarily reflex activities. What he lacked was the idea (the representation) of movement. As a result of this inability "il échoue complètement dans les épreuves plus compliquées, lorsque, placé en face de l'observateur, il doit imiter ses gestes, se mettre mentalement à sa place."

Since a good deal of our ordinary thinking involves the manipulation of spatial ideas, which, as the result of our education, have become unconscious, it follows that Head's tests are, to a certain extent, an indication of the mental level of the patient. Our experiments on school children bring this out clearly; for the difficulty of the task is such that bright children of the age of 8 years, who understand what is required of them, are nevertheless unable to cope with the problem. The ability to manipulate spatial ideas of this kind is an indication of the mental level attained by bright children at the age of 9 years. Duller children even at the physical age of 11 have not yet succeeded in mastering the task. For this particular problem a dull child of the age of 11 is at the same intellectual level as a bright child at the age of 8. Head's tests, therefore, form an exceptionally good means of gauging the intellectual disturbance in spatial ideas of patients suffering from aphasia. They provide an admirable method of measuring the exact stage of restitution of mental functioning in so far as this is concerned with the recovery of the spatial notions which lie at the base of so much of our thinking. They are probably more suitable to the advanced stages of re-education than to the exploration of the loss of function at the beginning of aphasia; and indeed Head partly employed them for this purpose, as he was in the habit of testing the same patients with the tests over a series of years.

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[1] W. van Woerkom, "La signification de certains éléments de l'intelligence dans la genèse des troubles aphasiques," *J. de Psychologie*, 1921.

V. DOES APHASIA IMPLY A DIMINUTION OF INTELLIGENCE.

The concept of aphasia has undergone many revolutionary changes and the present need, from the scientific standpoint, is for a more accurate differential diagnosis of its varieties. It seems to me that this could be secured if all the other serial tests employed by Head were standardized by being tried on school children; such as the naming of common objects, the naming and recognition of colours, the clock tests, the coin-bowl tests, etc. Many of these obviously necessitate the use of language, whilst others, as clearly, involve chiefly the manipulation of spatial and other relations. The purpose of the investigation should be to discover the lowest age at which the majority of normal children can cope with each serial test successfully. As it happens, a test for the naming of colours has already been investigated, for Binet employed it in drawing up his metrical scale of intelligence and it is now regarded as a test for the mental age of 5. Again, the ability to distinguish the right and left sides of the body, which is much simpler than the tests we have investigated, has been assigned to the mental age of 6[1]. In this way, by adopting the method used in the present research, a mental age could be assigned to every test and a scale drawn up; and it would then be possible to discover the precise degree of defect that different aphasics suffer from, and to estimate exactly the degree of restitution of function during recovery. The employment of graded tests by Head has considerably advanced our knowledge and the next logical step is to refine the graded tests by standardizing them.

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[1] Terman, *The Measurement of Intelligence*, ch. 12. Harrap.

The plan here suggested of determining the mental age of aphasics with regard to the various serial tests raises the difficult question as to whether the general intelligence of such patients is impaired. This obviously depends on what we mean by general intelligence. There is no doubt that several aphasics are highly intelligent people in spite of their handicap. One of the most striking examples is that of Dr Saloz, who was suddenly attacked with aphasia at the age of 60, and completely lost his powers of speech and writing[1]. On recovery, which was gradual and brought about largely by a deliberate and determined auto-education, he wrote an account of his own case. Immediately after his seizure he was able to make a correct diagnosis by pointing to his left temple to indicate to others what was the nature of the complaint. He says that he knew exactly what he wished to say, but had lost the symbols of expression. "Je me sentis comme muré dans un sépulcre." He had all his conceptions, but very limited means of expressing them to others. His thoughts, sentiments and volitions were intact, but there was a hiatus between his world of ideas and the world of words. His account reveals a very high degree of critical acumen, but, strangely enough, he sums up the various disabilities from which he suffered, thus: "La somme de tous ces déficits...équivaut à la perte ou à la faillite de l'intelligence." M. Naville, his friend and physician, says that, when he recovered, his intellect was not as supple as before, for he frequently adhered to a first opinion and had difficulty in following two ideas at the same time: defects which most of us share with him! M. Naville concluded that aphasia is not the result of diminished intelligence, but is characterized by the inability to employ a special mechanism that intelligence has created as its chief working tool.

Nevertheless, this view is not completely satisfactory for, after all, intelligence must employ some means of expression and the inability to make use of the recognized tools is *ipso facto* a loss of aptitude. We may best express this in the phrase employed by Hughlings Jackson, that the loss of the power to speak and write make a man "lame in thinking." There are, of course, alternative methods of arriving at a result; a child who cannot say what twice four are may, nevertheless, reach the answer by counting up on his fingers. Yet the inability to use the multiplication table would normally be regarded as a sign of a lower mental level. And our investigation has shown that what distinguishes the bright boys, *i.e.* those with most general intelligence, from the dull is the greater ability to deal with spatial relations. As there are a variety of other relations manipulated in thought the ability investigated is relatively specific. If there were no specific forms of intelligence there could not be such an entity as general intelligence. I am, therefore, in entire agreement with Dr Head who has summed up the situation lucidly[1]: "All the processes which suffer in aphasia are essentially psychical and, in so far as they are defective, the capacity of the mind is restricted. Yet it is not general intelligence which is primarily affected, but the mechanism by which certain aspects of mental activity are brought into play. In proportion as these forms of behaviour are necessary for the perfect exercise of psychical aptitudes 'general intelligence' undoubtedly suffers....Should the patient be unable to carry out a task in a normal manner, he falls back on a simpler and more childish method of solving the problem....But as with many children who are supposed to be mentally deficient, the loss of intellectual capacity is specific and not general."

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[1] Naville, "Mémoires d'un médecin aphasique," *Archives de Psychologie*, XVII.

A complete standardization of the serial tests of aphasia would help to determine the degree of loss and recovery of these specific capacities, and make the diagnosis more exact by providing a psychograph of each individual case.

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[1] H. Head, *Aphasia and Kindred Disorders of Speech*, I, 418. Cambridge University Press, 1926.

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THE INTERPRETATION OF CHILDREN'S DRAWINGS.

By C. A. OAKLEY.

(*From the Department of Psychology, Glasgow University.*)

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I. INTRODUCTION.

Two hundred investigations have been carried out in the field of children's drawings. They have produced little of practical value. Few have been analytical in method, most remaining satisfied with the airy pleasantness of descriptive writing. As a result spontaneous drawing continues to play little part in the standard systems of mental testing—an unfortunate position in view of its closeness to the child's interests, its relative freedom from the extraneous influences of both training and the personality of the examiner, its non-verbal character, its simple and ready application.

Summaries of these published works have been given by Ayer (*Psychology of Drawing*, 1916), Goodenough (*Psychological Bulletin*, 1928) and others. Recently there has been a renewal of the efforts to devise objective scales for the measurement of children's drawings, and it is with one of these attempts that I am at present associated. In the absence of standardized material, the collection of which must be a matter of some years, it has been suggested that the subject might lend itself to a discussion on the interpretation of these drawings.

II. EXISTING SCALES.

Scale-making has been approached from two directions—comparison and analysis (although it has been maintained that the former consists merely of doing crudely that which the latter seeks to do accurately). In methods of comparison the standards are median samples, there being generally one sample for each year or part of a year. Sometimes a series of samples is employed to each of which has been assigned a numerical value. In this case the drawing of the child under examination is marked, and by the means of an age-scale of normal scores, a (drawing) mental age is estimated.

On the other hand, for analysis, the product is resolved into its constitutive elements, and a score is based upon the quantity and quality of the details appearing in the child's drawing. A point-scale is arranged with a normal score for each year or part of a year.

Existing scales based upon comparison include those devised by E. L. Thorndike ("The Measurement of Achievement in Drawing," *Columbia Teachers' College Record*, 1913), by Cyril Burt (*Mental and Scholastic Tests*, 1921), by L. W. Kline and Gertrude M. Carey (*A Measuring Scale for Freehand Drawing*, John Hopkins Studies in Education, 1922), and by Stella McCarty (*Children's Drawings*, 1924). The only published example of the application of analytical method is that given by Florence Goodenough (*Measurement of Intelligence by Drawing*, 1926).

III. WHAT CHILDREN LIKE TO DRAW.

Each and every investigator has faced the fundamental difficulty of ascertaining the most suitable subject to be drawn by the child. Dr Beatrice Edgell has maintained that, when measuring the child's intelligence, we are on sound ground only when the tests follow the lines of his distinctive interests. This point is particularly applicable in the sphere of drawing.

Many investigators have followed Mrs Maitland (1895) in endeavouring to tell us what children like to draw. They are agreed that, in the earlier years, the subject of a human being predominates. (Ivanoff has called attention to the significant fact that of these representations 95 per cent. are those of adults and only 5 per cent. those of children.)

Typical of these general classifications is that given by Lukens (*Pedagogical Seminary*, 1896) for children of age up to 10 years:

	%
Human beings (or, including those in composite pictures, 75 %)	44
Illustrations of stories	12
Animals	7
Plants	3
Designs, etc.	2

Ballard has considered this question very thoroughly for children in South London and Glamorgan. He found that the boy's favourite object for drawing is a ship, a girl's being plant life, followed by houses. Ballard noticed that the age of loss of interest in drawing human beings is 9, there being a second falling off after the age of 12.

The work of these investigators may be summarized to bring out the following points:

(*a*) The child's favourite subject is that of a human being. (Drawings of ships, toyshops, etc., generally contain representations of people.)

(*b*) Houses and plants (for girls) and various mechanisms (for boys) are other popular subjects, drawn for their intrinsic interests and not as part of the illustration of a theme.

(*c*) Composite pictures take generally the form of either a representation of a favourite scene or an incident in a story.

(*d*) Environmental conditions influence the child's choice of subject. Ballard has shown that both Glamorgan and London boys draw ships. The same seems true of most British boys with their heritage of the sea traditions. But many investigators in other countries do not even include ships among the subjects drawn by boys.

The significance of social praise is discussed later.

IV. THE SELECTION OF SUBJECTS.

Only one investigator (Thorndike) has left the child free in his choice of subject. Constraint may be exercised in various ways:

(i) *Limitation to one subject.*

Both Burt and Goodenough restrict themselves to the examination of the child's drawing of a man and, if one subject only is to be employed, then this choice is more than justified. Other possible subjects include those of a man in character (for example, a postman), a dog, and a horse and cart. (The theme-illustration test, such as that given by Earl Barnes, is not considered. There is little record of any tests of this character being adequately standardized. Childs (*Journal of Educational Psychology*, 1915) gives an interesting test of this description, in which he employs a Thorndike Scale.)

(ii) *Utilization of more than one subject.*

The Kline-Carey test employs four objects—a house, a rabbit, a boy running, and a tree. McCarty limits herself to three—human beings, houses, and compositions involving trees. A numerical value is determined from the samples for each of the three drawings. The mean is calculated, giving the child's final score.

If we wish to employ the child's drawings to give us a measure of his intelligence, then, following Dr Edgell's line of criticism, we must dismiss the limitation method as inadequate. It favours the child who is accustomed to draw men.

In this paper, however, I am dealing only with one subject—that of the drawing of a man—and I leave further discussion of this point till a later page is reached.

V. INTERPRETATION OF THE SCORES.

Any discussion on the significance of a mental age based upon drawing must begin with an attempt to determine the extent to which intelligence is the basic factor.

Some interesting correlations between Binet (Stanford Revision) M.A.'s and Drawing M.A.'s are available. They may be tabulated in the following way:

(*a*) *By the Goodenough scale.*

Age	No. of cases	Correlation	P.E.
4	25	·863	·034
5	94	·699	·035
6	65	·832	·025
7	63	·716	·042
8	27	·557	·092
9	37	·728	·053
10	23	·849	·041

(*b*) *By the McCarty scale.*

4·0 4·6	47	·106	·097
5·0	183	·244	·047
5·6	239	·364	·038
6·0	308	·623	·024
6·6	214	·455	·037
7·0	220	·368	·039
7·6	107	·082	·065
8·0	75	·634	·047
8·6	27	·572	·088

The correlation is thus definitely positive, but the relationship, particularly in the case of the comparative method, is not of a high degree. Results obtained by the present writer are of the same order, and lead him to suppose that, speaking very generally, a correlation of between ·5 and ·6 is to be expected. A new scale is being standardized at the moment, but here again early indications are that no close correlation is obtainable. It would seem therefore that drawing tests do not provide us with a satisfactory measure of intelligence (assuming, of course, that the Binet tests themselves do so).

VI. HOW DO CHILDREN LEARN TO DRAW?

Two questions must be answered: (*a*) How does a child learn to draw? and, the more fundamental, (*b*) Why does he draw? Records of various attempts to analyse the drawing operation are available. Albien (*Zeitschrift für experimentelle Pädagogik*, 1907) distinguishes two distinct major processes, optical-perceptual (leading to the unconscious fusion or contrast of related ideas) and motor graphic. He analyses these processes and details the various factors which enter into them. Except for the emphasis upon visual imagery, there seems little in these processes which is not basic to, and constitutive of, general intelligence. He distinguishes also two drawing types:

(1) The visual type with two sub-types: (*a*) The good copy-drawer, whose visual image is clear, but transitory. Little 'apperception and association' enters into the activity. (*b*) The good drawer whose work is based upon analysis and subsequent synthesis.

(2) The constructive type which relies less upon visual imagery than upon reflection and subjective construction.

These have been compared to Messmer's objective (fidelity) and subjective (interpretative) reading types.

Spearman has shown that, while the eductive processes correlate highly with 'general intelligence,' the reproductive processes do not. As in the case of so many other writers, he confesses that he is baffled by the wide discrepancy which experimental results show between tests of visual discrimination and intelligence (*The Abilities of Man*).

In his earlier volume (*The Nature of Intelligence*) Spearman discusses imagination. He maintains that the elements distinguished out of the unity of completely undifferentiated confusion are apprehended by virtue of the second principle of eduction in relation to each other. The secondary unity does not spread itself evenly over the whole mental field. It settles in unrelated patches and, of these patches, the most conspicuous in early development is that of vision. This unity, says Spearman, would appear to be the main fact responsible for the "seeing of a picture from the very first as a whole."

He dismisses the extremist's conception of imagination as fashioning spontaneously out of nothing and as being emancipated from all laws. "The imagination is asserted by the majority of professed psychologists to do really nothing more than bring old cognitive elements into new combinations. Even the newness of the combination is represented as being merely the chance effect of associative reproduction." Certainly the source of cognition must be experience.

It is necessary, therefore, that the drawing processes should be examined in the light of Spearman's qualitative and quantitative principles which delineate respectively the cognitive operations and the extensity of their execution.

In the meantime, the writer assumes that the general level of the testee's intelligence must affect considerably his success at drawing. The immediate problem facing him is the explanation of the low correlation.

VII. WHY DO CHILDREN DRAW?

Meumann (*Vorlesungen zur Einführung*, 1907) points out that few people are sensible to the inaccuracy of their knowledge of the form and colour of the most familiar objects. The syncretism of which Piaget writes is remarkably evident in the confidence which we display in the exactness of our visual images. Among the possible explanations of this inaccuracy distinguished by Meumann are:

(1) The non-arousal of the will to analyse.
(2) The presence of the will, but the absence of the ability.
(3) The lack of schemata.
(4) The lack of knowledge.

Thus attention is called to the conative aspect of the question. If the child is not interested in a subject he will show little desire to analyse his visual impressions of it, and the quality of his drawings will be poor in comparison with that of those of subjects which do mean something to him.

Accepting as valid the argument that a child will only draw well those subjects which have interested him sufficiently to lead him to study and to analyse them, we have still to explain why he should want to *draw* them.

Interest in telling something is the motive which inspires all good drawing, say Sargent and Miller (*How Children Learn to Draw*, 1916).

This position is generally accepted. A child draws to express his ideas, drawing being a primitive and universal mode of expression.

Much has been written concerning the intuitive expression to be seen in children's drawings. Indeed they are quoted frequently as evidence of the intuitive origin of aesthetic appreciation. A very full discussion on child drawing and art is given by Helga Eng (*Kinderzeichnen*, 1927).

Looking at pictures plays an important part in the first years of childhood. We have the authority of Stern for stating that, at the age of 9 months, a child can recognize pictures. At 10 months, Eng's niece began to scribble. Pictures, says Stern, incite and guide intellectual processes.

Paullson (*The Creative Element in Art*, 1923) goes further: "The impulse in drawing is the pleasure derived from the objectification of emotion." When the child sees a picture 'At the Seaside,' he re-lives affectively his recent holiday. This suggests that the child who draws a toyshop or a ship is seeking to give completeness to his imagery. He might turn, of course, to the illustrations in his picture books, but this he does only when the books thrust themselves upon him. The child is never a good searcher.

Quite early in life drawing becomes a means of gaining social approval. A child will repeat with unvarying monotony a drawing of a flower which originally earned for him some praise. In this way the desire which first prompted him to sketch the flower may be quite superseded by the impulse of an affective memory.

It would seem that only during the years coming between the ages of 6½ and 10 does the child use drawing for expression. During this period he attempts to express himself to himself. In this his work does not differ materially from art proper although, in practice, the latter is very much distorted by the influences of social approbation.

At the best, however, the normal child finds the process a rather disappointing one. The number of unfinished drawings in childhood must exceed considerably the number of those completed. Sargent and Miller discuss as a possible reason for the fading of the early promise of childhood into apathy the growing realization of the crudeness of the representations. This is very noticeable after the age of 9 years has been reached. The disappointing results achieved in graphic expression seem to lead to its total abandonment during adolescence in favour of linguistic expression.

VIII. SCHEMATA AND ASSOCIATED DETAIL.

All drawings as made by the youthful scrawler and by the polished artist are built up on experience. Their existence is an objective fact. The visual schemata upon which they are supposed to be based is still merely conjectural.

It has been pointed out that drawing may be a motor rather than a visual activity, and a definite analogy between drawing and writing would seem to be permissible.

The nature of imagery is controversial ground. The accuracy and completeness of any explanation of the drawing act must depend upon a clarification of the general position. In the present instance the existence and fundamental importance of schemata, both visual and motor, is postulated.

It is suggested also, and our own introspection, unreliable as it may be, would seem to give some justification, that the dominating factor in the drawing process is the actual figure which is developing upon the paper. In the course of time, no doubt, if the same drawing is repeated indefinitely its execution becomes as mechanical, in the motor sense, as is walking up the stairs.

It is true also that an urge to create—a possible manifestation of self-assertion—prompts the child on the first occasion to scribble, and that the imitative action with the pencil is motor, not visual. But it is felt that only by reference to visual factors can one attempt to account for such unquestioned facts as the incidence of the mixed profile, the general stages of development in drawing, and the 'copying' of details which is a source of constant vexation to the examiner who would make of drawing a group test.

Thus it is said that, while drawing is based upon both visual and motor schemata, the former is predominant. Even in the case of the child who draws one standard figure with monotonous, and presumably mechanical, regularity, there is constant reference to its visual appearance as it passes through the various steps towards its completion.

The point must be left, however, in the absence of experimental and statistical evidence. In the meantime, it may be said that while some information is available on the character of the visual schemata, little is known of the motor schemata. But, just as the child's abilities are adequate for the demands made in the learning of the writing process, so are they in drawing. As is suggested above, the child before adolescence is not critical of the quality of his own work. He fails frequently to derive the satisfaction from it which he set out to achieve. He destroys, erases, and does not complete his work. But does he blame his own incompetence for this, or look upon drawing as a game not worth the candle?

The child analyses, in the light of his existing schemata, details in drawings, pictures and illustrations, which catch his attention. The synthesis of the schemata seems to take place as the child makes his drawing. The resulting visual schema tends to become more detailed. The point may be illustrated. It may be supposed that at the present time a child's schema of a man is as in Fig. 1.

In addition there is associated detail, the product of more recent analysis, not yet stamped into the schema. Its appearances fluctuate, and the final drawing may be as in Fig. 2.

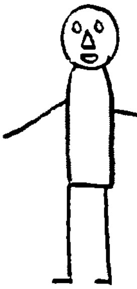
Fig. 1.

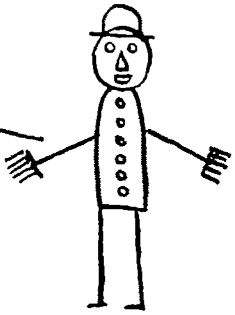
Fig. 2.

I suggest, therefore, that the child's drawing of a man is not based upon an analysis of reality. Its symbolism originates in drawings made by other people. The subject itself arises in the environment. The drawing of a man is at first simply a standard form of creative play, willingly accepted by the conservative child.

IX. GROWTH OF THE SCHEMA.

We have to explain accordingly the child's manifest liking in general for pictures, and, in particular, for those which are drawn for him. Various discussions on this point have been published, but none can be regarded as really conclusive. Different factors may be mentioned—the close social contact involved, the child's fascination with the rapid development of the figure, his self-assertive delight in knowing the detail which is coming next, his reaching out for his environment, his urge to create, his pleasure in his own skill.

One hesitates to compare scribbling to the half-automatic crying and babbling with which speech is inaugurated. The latter are certainly innate, and the organs exercised are essentially speech organs. Drawing is merely one of the innumerable accomplishments gained by the skilful control of the fingers.

The ordered course of linguistic development has been planned and plotted in numerous works. At first it would seem that this information already available would shed considerable light upon the present problem. Lukens attempted to show a complete parallelism.

In linguistic development the most important single element is the discovery by the child that everything has a name. In graphic development it would seem to be the first appreciation of resemblance or likeness—an interesting point in the light of Spearman's eductive principles.

The essential difference, rendering invalid any precise comparison, is the initiating motive. The young child uses speech to control and command. It is the medium for social expression.

No such fundamental need underlies the drawing act. It is true that many writers have maintained that the child of four years of age, when drawing, is really describing and communicating. It is possible on the other hand that the motives may be, when he is alone, those of self-amusement and self-assertion, and, when he is in the company of others, the showing-off of what he knows.

The most interesting of all the phenomena to be noted in the development of the child's drawing of a man is 'the mixed profile,' the spasmodic turning of details from the full-face to the profile representation. It appears with surprising regularity at the age of 6½ years, although some of the more recent American work would suggest that the deviation from 6½ is rather larger than is generally supposed. Thus McCarty gives the following percentages (based upon a very large number of cases):

Age ...	4 years		5 years		6 years		7 years		8 years	
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Full-face representation	92	94	89	91	67	76	51	64	53	64
Profile representation	1	1	5	5	21	16	32	24	37	28

Doubtful drawings, including mixed profiles, account for the remainders.

Before discussing the significance of the change from the full-face representation to the profile, it must be pointed out that this applies only to the drawing of a man in a detached situation. In the illustrations of a theme the figures are quite frequently shown full-face and their representations are less detailed.

The occurrence of the mixed profile in the drawings of normal children happens at an interesting stage of their careers. Piaget, in his study of child logic, has called attention to the importance of the months with which the seventh year ends (6½–7). The appearance of the mixed profile coincides also with the passing of play as the entire attitude towards life. There is greater contact with reality. The desire for fact, it has been said, forces back imagination.

Previously children's drawings have been play figures. Much has been made of their symbolism. But does the child realize that they are symbolic? Various writers have pointed out the false logic of assuming that, because the child can indicate a picture and say 'pig,' he has learned anything more than that the sound 'pig' is associated with the picture. Are we quite certain that picture-language has always a more direct connection with the qualities of the object denoted than has sound-language?

Does the child who insists that some scribble is 'Mama' see any resemblance in his picture to his mother?—or does he perceive a similarity simply to a drawing of 'Mama' which someone has made for him?

Too much should not be made of this point. The schematic process is one of integration, and the child must learn during these acts of synthesis that a connection exists between the whole and its constitutive elements. Spatial arrangement forces him to perceive the relationship between the figure and its parts.

The child has first to learn, therefore, that the two small circles not only represent eyes, but resemble them. It is suggested that this process, which may be described, following Burt, as one of descriptive symbolism, holds until the appearance of the mixed profile.

Of all the symbolic components of the child's full-face drawing of a man, the nose bears least likeness to reality. The nose in profile is outstanding. It is easily drawn and its appearance is surprisingly realistic.

Steps underlying the first appearance of the second nose seem to me to include:

(*a*) The child's perception of a profile-drawn nose in pictures which he has seen.

(*b*) His apprehension of this detail as something not appearing in his drawing.

(*c*) Its tentative inclusion.

(*d*) The realization that this is a *nose* and that its inclusion is logically inconsistent with the presence of another nose.

(*e*) The appreciation of the greater realistic accuracy of the new representation.

During the next three years the child attempts descriptive (or logical) realism. Some details—such as the buttons—which are readily represented full-face may resist the change to profile.

More significant is, however, the representing of men in character during this stage. Also there are many theme-illustrations, and in these, as has been indicated above, it is the action of the man rather than the man himself which the child is endeavouring to represent. Not only are there fewer details, but the representation is frequently full-face.

The sequence followed in graphic development may be shown in tabular form:

General development	Graphic development	Characteristic features (following Burt)	Motivation*
Birth: 2½ (Physical adjustment)	2 : 4 (Scrawl)	Imitation : repetition	Self-amusement (Assertion of power)
2½ : 6–7 (Playtime of life)	4 : 6½–7 (Play)	Descriptive symbolism	Conventional play
7–8 (Transition stage)	7 : 9½ (Dawn of realism)	Logical realism	Expression to self
8–9 : 12–13 (Angular age)	9½ : 11–14 (Regression)	(Failure of) visual realism	—
13–14 : 16–17 (Puberty)	14–15 (? Revival)	—	—

* Also social approbation during all stages.

The desires underlying the attempts at objectification may be classified also. They are operative mainly during the stage of the dawn of realism.

Character of drawing	Objectified desire
Forms (letters, figures, flags, etc.) ...	Social approbation
Plants (trees, flowers, etc.)	Originally possession (social approbation)
Animals (birds, insects)	Possession or presence (many are mere forms, *e.g.* the kindergarten cat)
Ships and mechanisms (human beings generally included)	Possession or fanciful self-assertion. (Social approbation plays a large part, for example, in the continual reproduction of ships)
Houses	Presence or fanciful self-assertion
Human beings	Play, and, possibly, some self-assertion
Scenes	Presence or recall
Human beings (characters)	Fanciful self-assertion or recall (become very mechanical with praise)
Illustrations	Recall (? largely motivated by social approbation)

An interest in things.

Thus to sum up the arguments given above, three fundamental factors enter into the drawing process:

(*a*) The ability to analyse and synthesize.

(*b*) The desire to apply this ability to a particular subject or group of subjects for the purposes of objectifying his desires. (Burt has suggested that this factor comprises (1) an interest in the visual appearance of things, and (2) an interest in reproducing these appearances.)

(*c*) The fineness of the quality of hand-eye co-ordination.

Of these the least important is (*c*). It is popularly supposed that the artist possesses exceptional kinaesthetic control, and that a comparative clumsiness limits his more normal fellows. That the artist is unusually skilful is beyond question, but practice and training play their part. Awkwardness will certainly discourage a child when he realizes that he is awkward. But it is a doubtful point if most children realize for themselves their deficiencies before they have been pointed out by the teacher and others. Self-criticism is not in evidence before adolescence (but consciousness of failure to win approval most certainly is).

The factor (*a*) is closely correlated in my opinion with intelligence. I have suggested that it is not affected by individual variations in the qualities of *innate* visual powers. If this assumption is incorrect, the argument falls.

The determining factor is (*b*). Thorndike and others have divided mankind into those who work with ideas and those who work with things—the word (or abstract) thinkers and the thing (or concrete) thinkers. Experimental evidence on this point is not encouraging, but that some such distinction does exist is assumed in this article.

Now drawing may be defined as the act of representing objects by means of lines. During, at the least, childhood, it is essentially concrete.

One is attracted by the hypothesis that the factors underlying success at drawing are (*a*) intelligence, (*b*) the desire to think concretely.

Burt has suggested a possible subdivision of the concrete thinkers into those who handle the realities (the mechanics) and those who think in terms of them (the draughtsmen and the artists). The trend of the day, with its ready supply of constructional toys, is probably towards the increase of the first group at the expense of the second. It may be pointed out that the young builder does seem to draw his motor cars and his ships fairly frequently and that they are generally peopled, so that the choice of a man as subject tends to bring the two concrete groups together. But the whole discussion must await experimental evidence.

X. THE MOST SUITABLE SUBJECT FOR DRAWING RECONSIDERED.

Developing the suggestion that the division between these two psychological types is fundamental, we can proceed to distinguish six drawing types:

Intellectually above normal ...	Word-thinker
,, ,, ...	Thing-thinker
,, normal	Word-thinker
,, ,, ...	Thing-thinker
,, below normal ...	Word-thinker (? non-existent)
,, ,, ...	Thing-thinker

At an earlier stage it was maintained that an intelligence test based upon the drawing of one subject (a man) was inadequate. Now, however, I suggest that the test should not be used for measuring intelligence. Rather should we begin possessing the child's I.Q., and endeavour to classify him as either a word-thinker or a thing-thinker.

The former, unlike the thing-thinker, will not score well. Therefore, to increase our chances of detecting him, we shall make our test as concrete as possible. A man is the ideal subject (or alternatively a theme with a man as central figure, *e.g.* "a gentleman and his little dog walking down the street: it is raining, so he has his umbrella up"). It has the added advantage of being remote from the influences of scholastic training. The schema is the product of integration. Practice (repetition) is essential. If I am justified in suggesting that the child who draws often cannot help but include men frequently, then, as a result, his schema of a man will be more fully developed.

The word-thinker of marked intellectual ability will do fairly well in the test. Unable to base his drawing upon an extensive schema, his general intellectual ability will enable him to summon much associated detail to his assistance. Thus one finds in these drawings unexpected features—unusual points scored, obvious points omitted.

XI. THREE SUGGESTED SCALES.

Goodenough includes over fifty points in her scale. But of these many are scored by no young children, and others are scored by all older children. The examining of drawings for these points is, therefore, rather laborious and monotonous. It is suggested that an improvement would be effected by drafting three scales with the appropriate points for (*a*) the pre-mixed-profile years (2 : 6½–7); (*b*) the pre-regression years (6½–7 : 9½–10); (*c*) early adolescence (9½–10 : 14–15). Each scale may be reduced to twenty points. Three drawings made on separate days or weeks should be examined.

The first scale (2 : 6½–7). The motive during these early years being assertion of power rather than expression, it follows that the drawing scores should correlate closely with intelligence. It will be noticed, however, that the correlation coefficients given by McCarty and Goodenough have little in common. Unfortunately the factors set out below as affecting success at the test are very marked at this stage. I suggest that we cannot apply a scale to the drawings of a child who has been in school less than three months and expect to obtain accurate measurement.

The second scale (6½–7 : 9½–10) is in my opinion the most useful of the three scales.

The third scale (9½–10 : 14–15). Unfortunately regression influences these drawings, but they should be of value in enabling us to devise a coefficient of regression throwing considerable light on individual problems of adolescence. Details worthy of special consideration in this respect are the treatment of hands, arms, difficult articles of clothing, general proportions, and lack of decisiveness in line.

XII. SOME FACTORS AFFECTING SUCCESS AT THE TESTS.

(1) Environmental conditions. The child learns to play the drawing game from others, and his skill is determined by their skill. His desire to play the game originates, to some extent, in their willingness to play it with him.

(2) The supply of pictures. Too much importance can be attached to this point which is fully discussed by Stern.

(3) The companionship of other children.

(4) Training in the use of a pencil (crayon work cannot be accepted).

(5) The influence of formal instruction which may have been given in the drawing of a man.

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MEMORY AND MENTAL IMAGERY.

AN EXPERIMENTAL STUDY[1].

By HENRY BOWERS.

(*High School, Fergus, Ontario.*)

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II. *The problem* (p. 272).
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I. INTRODUCTION.

No attempt will be made to review at length the extensive literature dealing with memory and imagery. Such questions as the influence on degree of retention of mode of presentation and type of material have been the subjects of much experimental study. In this connection the bearing of ideational type naturally came under consideration. However, criticism has been directed against the tendency to infer imaginal type from ability in remembering given material. This criticism is supported by the finding of Foster (3) that the work of observing visual material might be transferred to another sense department, and that such transfer might even be advantageous.

In a recent article (2) the writer drew attention to the existence of a relatively constant order of precedence in certain words isolated from context, with regard to visual imagery aroused by them in a group of secondary school pupils. (A similar condition was found to exist when words denoting sound were used.) Pupils were asked to rate their imagery as 3, 2, 1, 0, for each word, according as they experienced images ranging from very clear and distinct, through medium clarity to no image. The 'mean image' aroused by each *word* was calculated. These 'mean images' obtained from one group of subjects were correlated with those secured

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from another, yielding the Pearson r coefficients 0·86 ± 0·04 and 0·86 ± 0·03. From data secured in the present study the comparatively high coefficient 0·94 ± 0·01 was calculated.

II. THE PROBLEM.

It seemed that this establishment of a fairly constant order of precedence of certain words in respect of the imagery aroused by them in a given group of subjects afforded a new approach to the problem of influence of imagery on memory. Data were secured from which coefficients of correlation were calculated between images for 'visual,' 'auditory' and 'kinaesthetic' words claimed by a group of subjects and the numbers of times these words were recalled by the group.

III. OUTLINE OF METHOD AND CONCLUSIONS.

A list of 143 monosyllabic words whose concomitant imagery was likely to be visual was made up by taking three selections of the same forty-five words, each selection being in a different chance order. Four 'dummy' words were placed at the beginning and four at the end of the list. These 'buffers' were to obviate the effects of primacy and recency, and played no further part.

This list was read to groups of subjects in accordance with an objective technique previously developed and practised. Reproduction of the words in any order was then required.

'Auditory' and 'kinaesthetic' words were also employed.

After the reproduced lists were collected a brief talk on the relevant type of imagery was conducted. The subjects were then requested to rate the imagery aroused by each word on a scale of 4 (2).

Thus, for each word were known the 'mean image' claimed by the group, and also the number of times it was remembered. From these figures coefficients of correlation were calculated.

The rate at which the words were read was varied, and also the time elapsing before reproduction was requested in an attempt to discover optimum conditions for the functioning of imagery.

It was found that:

(1) A small but significant positive correlation existed between the numbers of times visual and kinaesthetic words were remembered and the imagery claimed for them.

(2) In the case of auditory words the corresponding coefficient was zero.

(3) Decrease of association between the words was without appreciable effect on the coefficient.

(4) The coefficient for words in the first third of the reproduction list was no higher than that for words in the last third.

(5) The relative constancy of imaginal content previously found for visual and auditory words was confirmed.

(6) A similar property existed in the case of kinaesthetic terms.

IV. GENERAL PLAN OF EXPERIMENT.

The general plan adopted was as follows. Three lists, each containing forty-five monosyllabic words, were formed, one of visual, another of auditory, and the third of kinaesthetic terms. From each of these lists a new one, containing 135 words was made, by a random selection of the forty-five words for its first third, a second random selection for its next third, and similarly for the last third. To this new list eight 'dummy' words were added—four at the beginning, four at the end. This effectually counteracted the effect of primacy and recency on memory scores for the forty-five experimental words. These 'dummies' had no further place in the experiment. The list of 143 words was read to a high or public school class, at a fixed rate—either 1 or 2 seconds per word, and in accordance with an objective technique, developed and practised in preliminary experiments. This procedure is not published here, but will be gladly supplied by the writer to any worker who wishes to repeat the experiments. It may be mentioned that the mean time error in reading is estimated at 3 per cent. To obviate as far as possible any tendency to speaking movements on the pupils' part, they were required to hold their tongues against their front teeth. In some cases the subjects wrote the words in any order immediately on completion of the reading, in others a 2-minute period intervened. This interval was employed in performing multiplication. The pupils were given all the time they required. When everyone had finished a brief talk was conducted on the appropriate type of imagery (see "Constancy of Imaginal Content"). Then sheets were distributed containing the word list and instructions for rating imagery aroused by each word. (The fact that this list had been read should be remembered when considering scores made after a 30-day interval.) Special warnings were given in connection with the estimation of kinaesthetic imagery to avoid imitative body movements, also not to confuse this imagery with mental pictures of people performing the various movements.

It seemed not unreasonable to suppose that the correlation between imagery for words and word score would be influenced by the time allotted to each word when reading the list. A longer period would presumably allow more opportunity for the arousal of images. There was also the possibility that the time-interval elapsing between reading and reproduction might have some bearing on the results.

Reference to Table I will make clear the several variations in procedure.

The following are the lists used, placed in alphabetical order:

Visual: beard, boat, clan, dime, elf, fiend, ghost, grass, hat, hen, jug, path, pew, phrase, plank, porch, pouch, prince, purse, quill, rib, rogue. rug, rust, scar, shrub, sky, slum, snake, swan, tent, thief, toad, tongs, tweed, twin, valve, van, verb, wad, wand, wart, weed, wig, wisp.

Auditory: bang, blare, bleat, buzz, call, caw, chime, chirp, clang, clap, click, cluck, coo, crash, crunch, cry, groan, growl, grunt, hiss, hoot, hum, mew, moan, moo, pop, purr, quack, roar, scold, scream, shout, shriek, shrill, slam, snore, splash, squeak, squeal, thud, toot, whine, yap, yell, yelp.

Kinaesthetic: bite, bump, chew, clasp, climb, cling, dance, dig, fall, fling, glide, grasp, grip, heave, hit, hop, hurl, jump, kick, leap, prod, pull, punch, push, ride, rub, run, scratch, scrub, shake, shove, shrug, slide, slip, speed, squeeze, strike, sweep, tear. tug, throw, twinge, twist, twitch, walk.

Circumlocution will be avoided by the use of the following contractions: (*a*) 'mean imagery for words,' to imply the mean imagery rating obtained from a group of subjects for each word; (*b*) 'imagery for subject,' for the total imagery rating of each subject on a given list of words; (*c*) 'word score,' for the number of times a given *word* was recalled by a group; (*d*) 'subject's memory score,' for the total score of a given subject in reproducing a particular word list.

In this article the following will be considered—the correlation between mean imagery for words and

(1) Word score.

(2) Word score, after the elimination of common associates.

(3) Word score, in the first one-third of each subject's reproduction list.

(4) Word score, in the last one-third of each subject's reproduction list.

(5) Word score, after the lapse of 30 days.

The following coefficients were calculated incidentally between:

(1) Imagery for subjects and subjects' memory score.

(2) Visual and auditory imagery for subjects.

(3) Auditory and kinaesthetic imagery for subjects.

(4) Kinaesthetic and visual imagery for subjects.

(5) Word scores obtained from two groups of subjects to whom words of the same kind (visual, auditory or kinaesthetic) had been given.

Table I. *Coefficients of correlation between imagery for words and (a) word score, (b) word score after partial elimination of associates, (c) word score for first third of reproduction list, (d) word score for last third of reproduction list, (e) word score after 30-day interval.*

					Pearson r coefficient of correlation between words and				
Type of words	Time per word in reading (sec.)	Reproduction (min.)	Grade	No. of subjects	Word score	Word score after partial elimination of associates	Word score for first third of reproduction list	Word score for last third of reproduction list	Word score after 30-day interval
Visual ...	2	Immediate	1st yr. H.S.*	31	·25 ± ·09	·29 ± ·09	·15 ± ·10	·08 ± ·10	·10 ± ·10
Visual ...	1	"	2nd yr. H.S.	36	·33 ± ·09	·28 ± ·09	·16 ± ·10	·18 ± ·10	·35 ± ·09
Auditory ...	2	"	1st yr. H.S.	38	·09 ± ·10	·07 ± ·10	·12 ± ·10	·04 ± ·10	·38 ± ·09
Auditory ...	2	After 2	Final yr. P.S.†	33	·11 ± ·10	·07 ± ·10	·01 ± ·10	·16 ± ·10	—
Auditory ...	1	After 2	Final yr. P.S.	34	·08 ± ·10	·08 ± ·10	·13 ± ·10	·04 ± ·10	—
Kinaesthetic	2	Immediate	1st yr. H.S.	39	·12 ± ·10	·11 ± ·10	·02 ± ·10	·26 ± ·09	·36 ± ·09
Kinaesthetic	2	After 2	Commercial (from 1st, 2nd and 3rd yr. H.S.)	30	·38 ± ·09	·35 ± ·09	·34 ± ·09	·36 ± ·09	·48 ± ·08
Kinaesthetic	1	After 2	3rd and 4th yr. H.S.	35	·24 ± ·09	·21 ± ·09	·14 ± ·10	·27 ± ·09	·39 ± ·08
Kinaesthetic	2	Immediate	2nd yr. H.S.	37	·39 ± ·08	—	—	—	·36 ± ·09

* High (secondary) school.

† Public (primary) school.

(6) Subjects' memory scores on visual and auditory terms, etc.

(7) Imagery rating for visual (auditory and kinaesthetic) words obtained from one group and that obtained from another.

V. CONSIDERATION OF A SERIES OF CORRELATIONS.

Correlations between mean imagery for words and word score.

Pearson r coefficients between mean imagery for words and word score are reported in Table I.

In the case of visual words the coefficients are small but positive. In view of the size of the probable error, it may be said that there is no appreciable alteration of the coefficient effected by changing the time from 1 to 2 seconds in reading visual words. Previous experiments, in which a rather different technique was employed, indicate that delay before reproduction exerts negligible influence on the coefficient. The average of four coefficients, when immediate reproduction was asked, was 0·27 ± 0·12, and that of four coefficients obtained with delayed reproduction was 0·18 ± 0·12.

Coefficients found with auditory words were much smaller. Taking into account the size of the probable error, the positive sign of all three may be merely a coincidence. Again, the effect of varying the time of reading, or the interval before reproduction was insignificant. The coefficients for kinaesthetic words were higher, averaging almost the same as those for visual terms. There is no apparent influence on the coefficient of variation of reading time or interval before reproduction.

Correlations between imagery for subjects and subjects' memory score.

It might be of interest, at this point, to record correlations between the sum of imagery ratings of pupils with their memory score. Since there could be no basis of comparison between the sum of the imagery ratings of one pupil on a given word list with that of another, zero correlation might be expected. This was the case. For visual words Pearson's r was − 0·09 ± 0·11, for auditory words 0·11 ± 0·11, for kinaesthetic terms − 0·11 ± 0·11. Thirty-four subjects were used in each correlation.

Correlations between mean imagery for words and word score after elimination of common associates.

One could not help being impressed by the frequency with which certain words were found paired together in the reproduction list. It would appear that the phrase 'words isolated from context' requires modification, for there is evidently a striving after artificial context when a natural one is not provided. Association seems to be a very potent factor in the recall of words 'isolated from context.'

Since associations are frequently made for reasons obviously unconnected with the imagery under investigation (*e.g.* 'red—wed'), it seemed possible that the diminution of association might render more apparent any connection between imagery and retention. Such decrease might be expected to result in an increase in *r*.

For the purpose of this study the writer makes a convenient division of association into 'common' and 'private.' Common association is word-linkage characteristic of the group, and due doubtless to the influence of a more or less similar environment. This type is detected when words pair together a number of times, considerably exceeding that expected on a chance basis. Private association is word-linkage peculiar to the individual and cannot be detected objectively. Even introspection will frequently be at fault in uncovering the more subtle private associations. The only distinction which concerns us here is the difference in methods of detection.

The following example will illustrate the device adopted for the detection and partial elimination of common association. Suppose the original list consisted of the following words: boat, arm, desk, red, man, tree, book, toad, snake, snow. These were arranged vertically and horizontally as indicated.

	—	Arm	Boat	Book	Desk	Man	Red	Snake	Snow	Toad	Tree
—	.	.	.	.	.	.	1	.	.	.	.
Arm	.	.	.	.	.	.	.	.	.	.	.
Boat	1	.	.	.	.	.	.	.	.	.	.
Book	.	.	.	.	.	.	.	.	.	.	.
Desk	.	.	.	.	.	.	.	.	.	.	.
Man	.	.	.	.	.	.	.	.	.	1	.
Red	.	.	.	.	.	1	.	.	.	.	.
Snake	.	.	.	.	.	.	.	.	.	.	.
Snow	.	.	1	.	.	.	.	.	.	.	.
Toad	.	.	.	.	.	.	.	.	1	.	.
Tree	.	.	.	.	.	.	.	.	.	.	.

Then, a reproduction list such as follows: red, man, toad, snow, boat, would be read: blank red; red man; man toad; toad snow; snow boat; boat blank; and entered in the proper square. It will be noted that concentration of any word at the beginning or end of the list can be detected. Each combination was considered a separate association, thus 'red-man' was considered separately from 'man-red.' The number of entries that might be expected in each square was calculated. It varied from 0·11 to 0·21. Accordingly, common association was held to exist whenever two or more entries occurred in any square. Since one entry necessarily could not indicate common association, it appeared fair to exclude only the excess of entries over one in such a square. Corrected totals were then found.

The writer is fully alive to the arbitrary nature of some of his assumptions, also to the fact that conclusions regarding the presence of association drawn from a comparatively small total number of entries must be only approximate. Yet, it should be remembered, that his object was to determine the effect on correlation of *diminishing* association.

Coefficients of correlation between imagery for words and word score are given in Table I. It will be observed that there is no marked tendency to increase over or decrease from the coefficient already discussed.

It may be asked: "Why should such associations as 'crash-bang,' 'slam-bang,' 'buzz-hum,' be eliminated? May not the association of these words be occasioned by arousal of imagery?" This is admitted, but the aim was to study retention of words isolated from context, or even, as far as possible, from artificial context. The serial nature of the words is an unfortunate circumstance, and diminution of the possible effects of this was the object of this part of the work.

Correlations between imagery for words and word scores in the first third and also in the last third of reproduction lists.

Betts (1) concludes that imagery is especially excited when an obstacle in thinking is encountered. This suggested the possibility that scores for words occurring at the end of the reproduction list might correlate more highly in the imagery than those for words at the beginning. Some support for such a possibility is afforded by the apparent marked increase in effort necessary for recall of those last written.

The coefficients given in Table I are no larger for the last third than for the first.

Correlations between imagery for words and word score after the lapse of 30 *days.*

After a 30-day interval those classes that were available were requested to write as many as possible of the words. The resulting scores were, of course, influenced not only by the writing of the words in the previous test, but also by the reading of them from the mimeographed sheets when imagery ratings were being made. Table I contains the coefficients calculated.

Contrasting these coefficients with those obtained from the original memory scores a small decrease is noted in the case of visual words, a substantial increase with auditory words, and a less marked, but nevertheless significant, advance with kinaesthetic words. It might be mentioned that in previous experiments an increase from 0·10 to 0·17 was noted after 20 days, using visual words. However, in view of the large probable errors it is best to await further evidence before attempting to draw definite conclusions.

Correlations between subjects' memory score on words of each type.

The figures obtained made possible the calculation of certain other coefficients, which, without direct bearing on the problem, are interesting enough.

Total scores for pupils on visual, auditory and kinaesthetic terms were employed. These coefficients were calculated, between:

Subjects' scores on visual and auditory words	·35 ± ·10
Subjects' scores on auditory and kinaesthetic words ...	·53 ± ·09
Subjects' scores on kinaesthetic and visual words	·51 ± ·09
Subjects' scores on kinaesthetic and visual words	·51 ± ·09

Correlations between word scores.

Coefficients between word scores obtained from one class and those from another were calculated:

Visual word scores ...	·56 ± ·06
Auditory word scores ...	·68 ± ·05; ·70 ± ·05; ·62 ± ·06
Kinaesthetic word scores	·57 ± ·07; ·64 ± ·06; ·74 ± ·05; ·63 ± ·06; ·74 ± ·05; ·63 ± ·06

It is to be noted that coefficients from word scores are markedly higher than those from subjects' scores. Just as there appears to be a relatively constant order of precedence in words in respect of the mean imagery attached to them by a given group, so there would seem also to be an order of memorial precedence. This property is not so marked in the latter as in the former. Whipple ((5), p. 201) remarks: "In word tests, certain terms are often found to have a special reproducibility, evidently by attracting attention in some way. Thus Binet and Henri found that the word *pupitre* (desk), though in the middle of a series, and hence unfavourably placed, was recalled in an unusually large number of cases."

The question as to the effect of partial elimination of associates on order of precedence in respect of memory might be raised. Earlier work of the writer points unquestionably to a decrease of the coefficient. The following pairs of coefficients are those for 'memorial content' before and after elimination of common associates: 0·50, 0·28; 0·80, – 0·05; 0·60, 0·54; 0·67, 0·58; or, taking the means: 0·64, 0·33. Such decrease indicates that the relatively high coefficients between word scores reported above may, in part at least, be due to common environmental influences.

It should be borne in mind that the word scores for visual, auditory, and kinaesthetic words were obtained under different circumstances. For example, the coefficient of 0·56 ± 0·06 given above for visual words was obtained by using scores from two classes in which a 2-second and a 1-second reading interval were respective experimental variations.

Correlations between subjects' imagery for different kinds of words.

Imagery ratings having been obtained for one class for visual, auditory, and kinaesthetic words, it was possible to calculate coefficients between subjects' ratings for the three pairs.

Visual and auditory imagery ratings	·73±·05
Auditory and kinaesthetic imagery ratings ...	·74±·05
Kinaesthetic and visual imagery ratings... ...	·77±·05

Coefficients obtained in previous work, using visual and auditory words only, were higher, viz. 0·80, 0·86, 0·76, 0·84.

High coefficients are reported by Betts (1) for similar measures.

It is submitted that at present no interpretation of these coefficients is legitimate, other than that they are due to consistent tendency of each subject to rate high, medium or low.

Correlations between mean imagery ratings for visual (auditory or kinaesthetic) words obtained from two groups.

No coefficients for kinaesthetic images were reported in "Constancy of Imaginal Content." Data secured in this study supply that deficiency and confirm those reported for visual and auditory words. These coefficients were calculated:

Mean imagery for visual words	·94±·01
Mean imagery for auditory words	·87±·02; ·84±·03; ·86±·03
Mean imagery for kinaesthetic words ...	·88±·02; ·92±·02; ·88±·02; ·88±·02; ·93±·02; ·83±·02

Mean memory scores for subjects.

Table II shows the mean scores made by pupils of two classes on different kinds of terms.

The order for memory for these words is: auditory, kinaesthetic, visual, both for immediate reproduction and after 30 days.

Table II. *Mean memory scores (immediate reproduction and after* 30 *days).*

Class	Reading interval (sec.)	Reproduction	Mean score Vis.	Mean score Aud.	Mean score Kin.	Mean score after 30 days Vis.	Mean score after 30 days Aud.	Mean score after 30 days Kin.
1st yr. H.S.	2	Immediate	9·10	14·70	11·53	1·10	4·50	2·30
2nd yr. H.S.	1	„	14·44	—	17·30	3·07	—	6·64

VI. REMARKS ON METHOD.

1. It may be objected that the groups employed are too small. An answer to this is provided by the coefficients obtained from the mean imagery ratings, and also from the word scores. For the former these were 0·94, 0·87, 0·84, 0·86, 0·88, 0·92, 0·88, 0·88, 0·93, 0·83. For the latter the figures were 0·56, 0·68, 0·70, 0·62, 0·57, 0·64, 0·74, 0·63, 0·74, 0·74, 0·74, 0·63. Surely a smoothing-out of individual variations is indicated not only by the magnitude of these *r*'s, but also by their relative constancy. It is hardly necessary to point out that the P.E. depends not on the number of subjects, but on the number of words. An increase in the latter would of course entail a compensating increase in the former.

2. An assumption underlying the study is that coefficients of correlation involving memory and imagery are comparable when obtained from forms of differing standards in high and public schools. Scrutiny of the figures in Table I lends a considerable degree of support to this hypothesis.

VII. CONCLUSIONS.

1. There is a small but significant positive correlation between word scores and mean imagery for words in the case of visual and kinaesthetic terms.

2. If auditory words be employed the coefficient is zero.

3. Partial elimination of associates from the list of recalled words has no appreciable effect on the correlations.

4. Scores for words found in the last third of the recalled list yield no higher coefficients with imagery than those in the first third. Assuming that the last words written are recalled with greater effort than the first, it would appear, either, that this effort does not consist in appeal to imagery or that, as Moore (4) suggests, it is recognition of the image rather than its distinctness that counts. Even in the latter event it is difficult to comprehend why recognition is not aided by clarity and impeded by obscurity.

5. The lapse of a month appears to result in a higher correlation between imagery for words and word scores in the case of auditory and kinaesthetic terms. The evidence is rather contradictory when visual words are considered.

6. As might be expected, total imagery ratings for pupils have zero correlation with pupil memory scores.

7. Marked correlation between pupil memory scores is reported.

8. Still higher correlations were obtained between word scores from two groups of subjects for words of the same kind.

9. There is evidence that, for visual terms, the partial elimination of association results in diminution of the coefficients referred to in (8) above.

10. The figures reported in "Constancy of Imaginal Content" are confirmed for visual and auditory terms. A similar condition was found to apply in regard to kinaesthetic terms.

11. Both for immediate reproduction and after a 1-month lapse the order for recall of the different kinds of words used was auditory, kinaesthetic and visual.

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ON THE SATURATIONS AND CHROMATIC THRESHOLDS OF THE SPECTRAL COLOURS[1].

By D. McL. PURDY.

(*University of Kansas.*)

(*From the Psychological Laboratory of Harvard University.*)

I. *Introduction* (pp. 283–289).
II. *Description of apparatus* (pp. 290–292).
III. *The colourless interval* (pp. 292–301).
IV. *The intensity required for maximum saturation* (pp. 302–304).
V. *The chromatic threshold with respect to white* (pp. 304–312).
VI. *Summary* (pp. 312–313).

I. INTRODUCTION.

WORKERS in the domain of colour vision have generally been concerned with the problems relating to the perception of hue, to the neglect of another, less impressive aspect of visual sensations, namely, saturation. No theory of colour vision, however, can be regarded as adequate if it does not account for the laws of saturation as well as for those of hue. Our knowledge of the former laws is unfortunately rather meagre. Experimental work in this field might be expected to lead to a broader and more inclusive conception of the visual mechanism than we possess in the present theories, which are based upon a more restricted group of facts. One of the most fundamental problems of the psychophysiology of vision is that of the relationship between the chromatic and achromatic processes; this is one of the main points, for example, upon which Helmholtz and Hering disagreed. A careful study of the phenomena of colour saturation might be expected to throw light upon this important question.

The word 'saturation,' according to the prevailing usage, stands for a strictly subjective aspect of visual experience. Sensations of vision, it is generally agreed, have three attributes, namely brilliance[2], hue and saturation. All of these attributes are *defined* in psychological, not in physical or physiological terms. They may each have a certain correlation with aspects of the physical stimulus and with aspects of the physiological response, but this correlation is a matter of empirical proof or of assumption, and is not involved in the definition of the attribute in question.

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[1] The writer is grateful to Dr L. T. Troland for advice and criticism, as well as for the opportunity to use the latter's colour-vision apparatus in this research.

[2] The term 'brightness' is undesirable in this connection, on account of the established usage of the same word in a different sense in photometry. Cf. L. T. Troland, "Report of the Colourimetry Committee of the Optical Society of America," *J. Opt. Soc. Amer.* 1922, VI, 531.

The definition of saturation which has been adopted by the Colourimetry Committee of the Optical Society of America, and to which I shall adhere in this paper, is the following: "Saturation is that attribute of all colours possessing a hue, which determines their degree of difference from a grey of the same brilliance[1]." This is a strictly psychological definition.

A good deal of confusion has arisen through the practice of Helmholtz and other physicists who employ the word 'saturation' not in the sense of an attribute of sensation but as a description of the physical stimulus, or of the hypothetical physiological process which gives rise to sensation. Thus the word 'saturation' is frequently used by Helmholtz and his followers to signify the relative amount of achromatic component in the retinal response, as inferred not from direct inspection, but from the laws of trichromatic mixture. At times, however, Helmholtz used the same word in a different sense, *e.g.* in speaking of the 'saturation' of a contrast colour or of an after-image. It is obvious that if we choose to define saturation in some sense other than the psychological, we require some different word to describe the psychological characteristic of sensation.

It may be said that this distinction is unimportant, on the ground that Helmholtzian saturation and psychological saturation vary in strict parallelism with each other. But such a parallelism cannot be taken for granted until it has been experimentally established, and up to the present time the requisite experimental proof has never been performed[2].

The comparison of saturations, in our sense, is a very difficult matter, except under one special condition, namely when the colours to be compared have the same hue and brilliance. Tschermak goes so far as to say: "Die Frage, ob die verschiedenfarbigen...Eindrücke spektraler Lichter *dieselbe* Sättigung besitzen oder nicht, ist schwer zu beantworten[1]." But I believe that most observers will agree with me in finding no difficulty in seeing that the spectrum is not equally saturated throughout, and in particular that the red and violet are decidedly more saturated than the yellow. In my own case I am able to say that the red (wave-length greater than $670m\mu$) is clearly more saturated than any other spectral hue of equal brilliance, at any rate when the intensity is not above 200 photons.

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[1] Troland, *op. cit.* p. 534.

[2] There is a variety of possible ways of defining Helmholtzian saturation, and these lead to as many different quantitative results; cf. I. Runge, "Die Einheitsmengen in Maxwell-Helmholtzschen Farbendreieck und die Bestimmung der Farbsattigung," *Zschr. f. Instrumentenkunde*, 1929, XLIX, 600. According to the simplest and most customary definition, saturation is the ratio of achromatic to chromatic excitation. The experiments of W. Seitz ("Über die Definition der Sättigung einer Farbe nach Helmholtz und Exner und über das Ostwaldsche Farbensystem," *Zschr. f. Sinnesphysiol.* 1923, LIV, 146), indicate that saturation as thus defined is in pronounced disagreement with psychological saturation.

In the present study I have not attempted the awkward task of directly comparing the saturations of colours of unlike hue and brilliance, but have confined myself to certain problems which are more amenable to experimental attack. The first of these problems is that of the variation of saturation with the intensity of the stimulus.

As the intensity of a chromatic stimulus is increased, the experienced colour undergoes characteristic changes in saturation. At very low intensities, under ordinary conditions of vision (where both rods and cones function), all sensations are completely de-saturated, and the spectrum is simply a band of grey. As the intensity is increased, hues begin to appear. There is a definite 'colourless interval' between the 'general threshold,' or the intensity which just suffices to produce a sensation of light, and the 'specific threshold,' or the intensity at which colour is just noticeable[2].

The saturation of the colours steadily increases with rising intensity until a certain level is reached at which the saturation is a maximum. Further increase of intensity results in a diminution of saturation, and, according to the generally accepted notion, if the intensity is made sufficiently high the colours once more become completely de-saturated, and the entire spectrum appears white.

It is held by some authorities (*e.g.* König and Nagel) that the colourless interval is strictly a phenomenon of scotopic vision. The rods function at an intensity which is too low to excite the cones and yield an achromatic sensation. One would expect then that when the stimulus is confined to the central rod-free area of the retina, no colourless interval will exist. König, Nagel and von Kries maintain that this is in fact the case when spectral colours are observed. But other experimenters have obtained contrary results, and the question is still controversial[1].

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[1] A. Tschermak, "Licht- und Farbensinn," *Bethes Handb. d. Physiol.* Berlin, 1929, XII (1), 366.

[2] I am using von Kries's terminology. Some writers use the terms 'photochromatic interval,' 'absolute threshold,' and 'chromatic threshold.' The use of 'chromatic threshold' in the present connection is avoided on account of a possibility of confusion with another sort of chromatic threshold which is to be discussed later.

The recent careful experiments of Miss Monroe[2], in which a field of 2° 2·2′ was employed, point to the existence of a definite foveal colourless interval for the spectral colours. The longest wave-length she employed was $655m\mu$. Some experimenters have reported that the colourless interval is absent for wave-lengths greater than this value, in the red end of the spectrum.

According to Burch[3], the existence of a foveal colourless interval under ordinary conditions of observation is due to the presence of 'dazzle-tints'—positive after-effects of stimulation which take a long time to subside. When dazzle-tints are eliminated by resting the eye in darkness for two hours, stimuli appear coloured even at the threshold of vision.

Abney and Watson[4], in 1916, performed some interesting experiments which would suggest a reconciliation of the conflicting statements of different experimenters with regard to the central colourless interval. These writers maintain that there is one class of observers who perceive such an interval, and another who do not. Now, when the same observers are subjected to other tests, it is found that for the first class there is lacking that sharp demarcation between the phenomena of peripheral and central, or rod and cone vision, which exists for the second class. Abney and Watson draw the conclusion that the centre of the retina is not invariably a rod-free region, but that some persons possess a quite appreciable proportion of foveal rods. For these subjects, foveal stimuli of threshold intensity excite only the rods, and hence appear colourless. The foveal colourless interval, like the peripheral, is a phenomenon of scotopic vision.

The spectral colours lose their saturation not only when the intensity is very low, but also when it is very high. It is generally supposed that at some sufficiently high intensity this loss of saturation is complete for all hues, although this supposition does not seem to have been rigorously verified by experiment. Observation with exceedingly intense stimuli is difficult, and the results are complicated by the action of chromatic fatigue, which becomes more rapid and more pronounced as the intensity is increased. One author, indeed, believes that fatigue is responsible for the whiteness of the high-intensity spectrum, and that the apparent whiteness in the green region, for instance, is only "a transitory stage in the production of green-blindness[1]."

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[1] Cf. Sir J. H. Parsons, *Introduction to the Study of Colour Vision*, 2nd ed. (Cambridge, 1924), pp. 66 ff.

[2] M. M. Monroe, "The energy value of the minimum visible chromatic and achromatic for the different wave-lengths of the spectrum," *Psychol. Monog.* No. 158, 1925.

[3] G. J. Burch, "On colour vision by very weak light," *Proc. Roy. Soc.* B, 1905, LXXVI, 199.

[4] Sir W. de W. Abney and W. Watson, "The threshold of vision for different coloured lights," *Phil. Trans. Roy. Soc.* A, 1916, CCXVI, 91.

Even though fatigue be responsible for the ultimate whiteness of the spectral colours, it cannot be the sole cause of their decline in saturation. For, as we shall see later, this decline begins at very moderate intensities, where fatigue acts in relatively mild and slow fashion.

Many writers attach considerable importance to the question whether colours become completely achromatic at the two extremes of the intensity scale. For example, Burch thinks he has refuted Hering's theory by showing that the de-saturation is not, in fact, complete in the case of the spectral colours. Nagel apparently feels that an important difficulty for the duplicity theory is removed if it can be shown that the spectral hues display no colourless interval in the fovea.

On the other hand, no one denies that the spectral colours have very low saturation when the light is weak, and also when it is glaringly bright. It is universally agreed that saturation increases with intensity up to a certain point where it reaches a maximum, and then declines as the intensity is raised beyond this point. The present writer believes that this general behaviour of colours presents the really significant problem here, and that the question of the limiting values of the de-saturation effect is by no means one of fundamental importance. Even if there be no complete de-saturation, the problem of explaining a partial de-saturation is essentially the same.

Furthermore, it should not be thought that the behaviour of the spectral colours in this respect is of all-important significance. Although there has been disagreement about the existence of a foveal colourless interval for the colours of the spectrum, it is an undisputed fact that such a phenomenon exists for ordinary unsaturated colours (*e.g.* pigment colours). There is no reason to suppose that spectral colours must be used for a crucial test.

Let us consider the interpretation of the saturation changes which is offered by Helmholtz. According to his theory, the white sensation corresponds to equal amounts of the elementary 'red,' 'green' and 'blue' processes. A colour is saturated in proportion to the degree of predominance of the two strongest excitations over the weakest excitation, that is to say, in inverse proportion to the quantity of white which it contains. Helmholtz[1] accounts for the de-saturation of colours at high intensities in the following way. He assumes that chromatic excitations do not increase in proportion to their stimuli, but rather obey a law of diminishing returns (Fechner's law). This means that, for a given increase of stimulus, the weakest component of the retinal response will increase relatively more than the two stronger components. Accordingly, the colour becomes more whitish as the intensity is increased.

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[1] G. J. Burch, "On artificial temporary colour-blindness, with an examination of the colour sensations of 190 persons," *Phil. Trans. Roy. Soc.* B, 1899, cxcix, 50.

This hypothesis, however, would demand a continual decrease of saturation with increasing intensity, and indeed the saturation of a colour should be maximal at the threshold of vision. Actually, on the contrary, the saturation is very low at the threshold and only becomes maximal at a medium intensity.

Hering[2] explains the saturation changes in terms of the different sensitivities and the different quantities of the visual substances which his theory postulates. The 'white-black' substance is the most sensitive, and is decomposed at intensities which are too low to affect the two chromatic substances. Thus the spectrum appears grey in the neighbourhood of the absolute threshold. At very high intensities the available quantities of the three visual substances become exhausted. The chromatic substances, being present in smaller amount, are used up before the achromatic substance has reached its limit. Accordingly the spectrum approaches white as the intensity becomes very great. The maximum of saturation would be found at some intensity which is neither too great nor too small.

Thus the Hering theory is able to give a reasonable explanation for the general course of the saturation changes. Unfortunately, however, there is no way of formulating the predictions of this theory in quantitative terms. The Hering theory offers no basis upon which we can calculate the relative saturations of colours[3], and thus leads to no definite consequences which we can test by experiment.

It appears then that neither of the classical theories of colour vision offers us a fruitful working hypothesis for our present problem.

Although it is impossible to describe completely the changes of saturation with intensity in a quantitative fashion, there are two critical points in this series which are of particular importance and which can be experimentally determined. The first of these is the specific threshold, the lowest intensity at which colour is visible. It is of interest to know whether this point coincides with the general threshold, or whether there is a colourless interval, in the case of the rod-free area of the retina. A further question is whether this interval exists for all wave-lengths and, in general, what relative values it assumes for the various wave-lengths of the spectrum. This question is the starting point for my first series of experiments.

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[1] H. von Helmholtz, *Treatise on Physiological Optics* (Eng. trans., New York, 1924), II, 185 f.

[2] Cf. H. Ebbinghaus, *Grundzüge der Psychologie*, 3rd ed. (Leipzig, 1911), I, 275 f.

[3] Cf. Tschermak, *loc. cit.*

A second critical point is the intensity at which the saturation of a colour attains its maximum value. My next group of observations consists of measurements of this intensity for the different spectral colours.

The final series of experiments constitute a less direct approach to the problem. The comparison of brilliance is difficult when the colours in question have unlike hue and saturation; the difficulty is of the same type, though of less degree, as that involved in saturation judgments. We can, however, study the functioning of the brilliance mechanism by various indirect methods, such as the flicker method, which obviate the uncertainty of the direct method. This suggests the possibility of using some analogous procedure in the study of saturation.

If to a given coloured light we add a sufficient quantity of white light, the sensation of colour will be extinguished. For a fixed brilliance of coloured light one might reasonably expect the quantity of white light required for extinction to vary with the saturation of the colour, and in precisely the opposite sense. It might be presumed that the ratio between the intensity of the extinguishing white and the intensity of the coloured light would serve to describe the saturation of the colour. In that case we should have a perfectly general method for the comparison of saturations, regardless of hue and brilliance. In terms of this scale it would be possible to specify not only the relative saturations of the different spectral hues, but also the variations of saturation with the intensity of the stimulus.

With this idea in mind, I have made a series of determinations of the sort described for the various colours of the spectrum. Discussion of the meaning of these measures will be postponed until the results themselves have been reported.

II. Description of apparatus.

The apparatus used in these experiments is designed to present a bipartite circular stimulus field to the observer's eye at E (Fig. 1). The left half of this field may be illuminated either by monochromatic light from the spectrometer S_1 or by white light from the lamp Z. The right half of the field may be illuminated by the spectrometer S_2. Separate illumination of the two halves is accomplished by means of the Lummer-Brodhun prism P, which is silvered upon the right half of its diagonal surface.

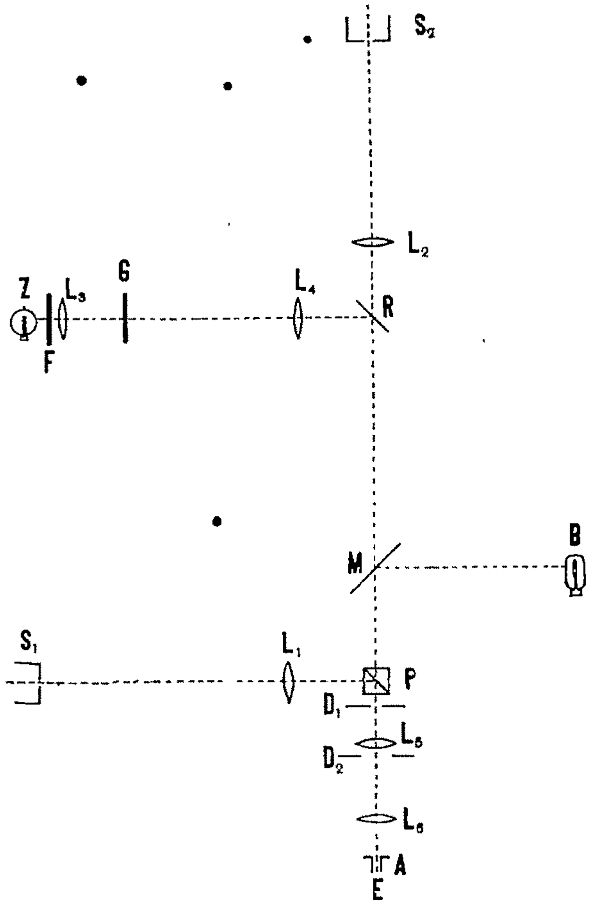

Fig. 1. Diagram of apparatus.

The collimator slits of the two spectrometers are illuminated by small incandescent lamps. By means of a slit at the ocular end, a narrow band of radiation is isolated from the spectrum of each instrument. These approximately monochromatic rays are made parallel by the lenses L_1 and L_2, respectively, before entering the prism. After emerging from the prism, these rays are converged by the lens L_5 in such a fashion that images of the two lamp filaments are formed, side by side, in the plane of the observer's pupil at E. The observer, looking through the artificial pupil A, sees an illuminated area when both spectrometer slits are illuminated. The colour of the left half of this area is due to S_1 and that of its right half to S_2. The size of the area is determined by the diaphragm D_1, which is just in front of the prism. The purpose of the lens L_6 is to assist accommodation.

A minute ink-spot on the centre of the lens L_5 serves as a fixation point. When very weak stimulus intensities are employed, a luminous point is substituted.

The lamp Z is provided with a blue filter F which renders its rays achromatic. The lens L_3 converges these rays to a focus, after which the lens L_4 makes them parallel. Finally, they are reflected towards the observer's eye by the glass plate R. A ground glass G is situated within the converging bundle from L_3. The intensity of light entering the eye can be varied by moving this glass towards or away from the focus. It is thus possible to change the intensity of the light without affecting its purity[1].

The lamp B, which is mounted upon a photometric bench, serves to measure intensities of stimulation. M is a magnesium-oxide sector disc with two open quadrants; its plane is inclined at 45° to the line of sight. Light from B reaches the eye by virtue of reflection at the surface of this disc. The disc may be rotated by means of an electric motor, and photometric measurements may thus be made by the flicker method. Where less accuracy is sufficient, the disc may be turned so that it furnishes steady illumination in the right side of the field, and the intensity received from S_1 may be measured by the equality-of-brightness method.

The intensities of the spectral stimuli in these experiments were controlled by varying the voltages across the lamps. Stimulus intensities were measured in terms of the actual illumination of the retina. The effective pupillary size was held constant by the use of the artificial aperture *A*, the breadth of which was less than the smallest diameter of the natural pupil. The measurements were expressed in terms of the photon, the unit introduced by Troland, and defined as that intensity of retinal stimulation which accompanies the use of a pupillary area of 1 square millimetre and an external stimulus brightness of one candle per square metre.

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[1] Before experimentation was begun, the illumination furnished by the lamp Z was matched with sunlight, introduced into the right half of the field by means of a heliostat and appropriate mirrors. The match was obtained by a suitable choice of filter and adjustment of lamp voltage. Thereafter the voltage was always held constant at this predetermined level whenever this lamp was used.

III. THE COLOURLESS INTERVAL.

Preliminary observation indicated that, in my own case, a colourless interval existed for all wave-lengths except those in the red end of the spectrum. This was true no matter whether the stimulus field had an area of 3°, 1½° or ½°. The red hues showed a behaviour decidedly different from that of the others; these hues had a very distinct saturation even at the threshold of visibility. But this saturation was by no means equal to that produced by the same wave-lengths at medium intensities.

In order to determine whether other observers experienced a foveal colourless interval, the test devised by Burch was applied. The field (with a size of 1·5°) was illuminated with light of different wave-length on the two sides. For a first test the stimuli used were blue ($470 m\mu$) and yellow ($575 m\mu$), and for a second test they were green ($505 m\mu$) and violet ($430 m\mu$). To ensure purity of stimulus, monochromatic filters of the appropriate transmissions were placed in front of the spectrometer slits. These stimulus combinations, blue-yellow and violet-green, were chosen as providing maximum contrast and optimum conditions for the detection of a difference in hue. I began the experiment by adjusting the stimulus intensities until both sides of the field looked grey to me, and were of approximately equal brilliance. A luminous fixation point was used to assist the observer in maintaining central regard. After my subject had undergone 5 minutes' dark adaptation he was asked to observe these weak stimuli and to report what hue he saw in each side of the field. The actual wave-lengths employed were unknown to him.

In order to test for the existence of a colourless interval in the red, one side of the field only was illuminated, the wave-length being $700 m\mu$. The observer fixated the luminous point, while I gradually increased the stimulus intensity until the semicircular field was just visible to him. At this moment he made a judgment as to colour.

The observers were Dr Karwoski and Messrs Harris, Oberlin and Volkmann of the Harvard Psychological Laboratory. In the case of every observer a low intensity could be found at which the 'blue-yellow' and 'violet-green' fields looked uniformly grey. In the case of the stimulus of

$700m\mu$, the observer invariably returned an emphatic answer that the field was distinctly red so long as it was visible at all. When he was asked to take special care to detect the existence of a possible grey phase as the stimulus intensity was gradually made subliminal, he always declared that no such phase existed; although with other colours such a phase was observed.

Thus for these four observers, as well as for myself, a foveal colourless interval is present in the yellow, the green, the blue and the violet, but is absent in the long-wave red.

In my own case I find that the specific threshold is lowered during the first few minutes of dark adaptation, but at the end of 5 minutes it reaches a constant value. A further 25 minutes of dark adaptation gives no measurable change in the threshold.

I have repeated Burch's experiment, already mentioned, on the effect of resting the eye in darkness for a prolonged period of time. As he recommends, I employed a bipartite field of two hues. For this purpose I chose green ($505m\mu$) and violet ($430m\mu$). As in the experiments with the colour pairs which I have described above, monochromatic filters were placed before the spectrometer slits, and the field size was 1·5°. To begin with, the intensities were lowered until the field looked uniformly grey. I then remained in darkness for 2 hours, and at the end of this period observed the same stimuli once more. I found that the bipartite field presented the same appearance as before; there was no suggestion of a hue on either side of the field.

Thus I am unable to confirm Burch's observation, although the spectral stimuli which I used would offer a more favourable condition for the appearance of hue than the less saturated coloured glasses which he employed.

I next determined, for my own eye, the relative magnitude of the colourless interval in the different parts of the spectrum. For a preliminary estimate, and as a check upon later quantitative measurements, the following simple procedure was followed. Pairs of stimuli of neighbouring wave-length (differing by about $10m\mu$) were presented in juxtaposition, and for each member of the pair the intensity was adjusted to the lowest value that would yield a sensation of hue. These two threshold stimuli were then compared in respect of brilliance.

One can thus observe how the specific threshold varies with wave-length. Since the general threshold may be regarded as constant, the colourless interval will vary in the same sense as the specific threshold.

In this way I found that the colourless interval steadily increases from 625 to 580$m\mu$, but has approximately the same value for 580 and 570$m\mu$. From 570 to 560$m\mu$ there is a slight increase, but the interval scarcely changes throughout the spectral region between 560 and 490$m\mu$. In the blue and violet, from 490 to 430$m\mu$, there is a continual slight decrease, but never a complete abolition of the colourless interval.

For wave-lengths greater than 625$m\mu$, in the orange and red, the interval is very small. I have attempted to determine the critical wave-length at which the interval just begins to appear, as one passes from the spectral red towards the yellow. Starting with 700$m\mu$, where it is definitely absent, I decreased the wave-length of the stimulus by small steps. After each step I adjusted the intensity of the stimulus until the semi-circular field was barely visible, and made a judgment as to the presence or absence of hue. In order to ensure steady fixation, a luminous fixation point was used.

I found that hue was visible at the general threshold until I reached a point in the orange-red, at about 665$m\mu$. The mean variation in determining this point was about 2$m\mu$.

Finally, I have measured, in terms of the photon, the magnitudes of the general and specific thresholds for the different wave-lengths of the spectrum.

For a determination of the general or absolute threshold, the right half of the field was illuminated by the spectrometer S_1, and a luminous fixation point was provided. A magnetic shutter was placed so that, by closing a switch, one could cause it to intercept the rays from the spectrometer. When the intensity had been adjusted so that it was near the threshold value, the switch was alternately closed and opened, causing the faint spot of light to disappear and reappear. The threshold was taken as the lowest intensity at which a definite disappearance and reappearance could be observed. This procedure, due to Reeves, provides a valuable control upon absolute threshold determinations, which are peculiarly subject to errors of imagination. The general threshold was determined for thirteen wave-lengths ranging from 700 to 430$m\mu$.

Each determination of the threshold was preceded by five minutes' dark adaptation, experience having shown that after this time the values were practically constant. The 'ascending' and 'descending' thresholds were measured alternately. The lamp voltage corresponding to the threshold was read after each measurement. A graph representing *log* photons as a function of *log* volts had been plotted for each wave-length employed. This graph, throughout the range of voltages in question, is sensibly a straight line. Accordingly, the threshold voltages could be translated into photon values by extrapolation upon this graph.

The specific threshold was also determined for the same wave-lengths (with the exception of 700$m\mu$, at which value the specific and general thresholds coincide). In every case this determination was made by comparison with a grey standard presented simultaneously on the other half of the field and having an intensity of approximately 0·02 photon. This standard was furnished by the light of the lamp *Z* which was rendered achromatic in the manner already described. The value 0·02 photon was chosen as providing approximately optimal colour sensitivity, and minimal thresholds.

It must be borne in mind that the results obtained refer only to the particular conditions, including size of field and intensity of grey standard, which were used in these determinations. In particular, if observations were made in the absence of any standard light (as was the case in the experiments of Miss Monroe), a quite different set of values would have resulted. Thus, Miss Monroe remarks:

> The chromatic threshold for red is by far the easiest to determine. The transition from achromatic to chromatic is abrupt and sharply marked and there is little or no hesitation on the part of the observer as to whether or not there is any colour present. ...In the yellow just the opposite condition obtains. The colour comes in very slowly and gradually and the exact point of appearance of the chromatic component is difficult to determine. At low intensities light of wave-length 580$m\mu$ has much the appearance of ordinary artificial light, such as that emitted by a carbon lamp, which an unpractised observer is accustomed to consider as white. In such a case much greater intensities are needed for the light to be called coloured.

My purpose was to obtain conditions of maximum sensitivity and minimum variability, and a grey standard was therefore desirable.

The photon values of the threshold stimuli were obtained as before by the use of the voltage calibration curve.

The results, which are tabulated below and graphically represented in Fig. 2, refer to six observations of the general threshold, and eight of the specific threshold, for each wave-length. The 'ascending' and 'descending' thresholds were determined alternately.

The general threshold has an average value of about 0·005 photon. The specific threshold, which is zero in the red, rises rapidly in the orange, and shows no further pronounced variation in the remainder of the spectrum. There is however a slight decline in the violet. For the greater part of the spectrum the colourless interval lies between 0·01 and 0·02 photon.

Table I.

Wave-length $m\mu$	General threshold		Specific threshold		Colourless interval
	Mean	Mean variation	Mean	Mean variation	
	Photons				
700	0·0023	0·0009	0·0023	—	0
625	0·0043	0·0003	0·0052	0·0012	0·0009
600	0·0017	0·0003	0·0071	0·0025	0·0054
585	0·0032	0·0003	0·0178	0·0054	0·0146
560	0·0033	0·0005	0·0174	0·0035	0·0141
540	0·0053	0·0005	0·0238	0·0032	0·0185
525	0·0035	0·0016	0·0244	0·0048	0·0209
515	0·0070	0·0032	0·0229	0·0030	0·0159
500	0·0074	0·0013	0·0237	0·0029	0·0163
490	0·0072	0·0014	0·0228	0·0026	0·0156
470	0·0078	0·0010	0·0221	0·0023	0·0143
450	0·0049	0·0010	0·0179	0 0044	0·0130
430	0·0040	0·0010	0·0191	0 0009	0·0151

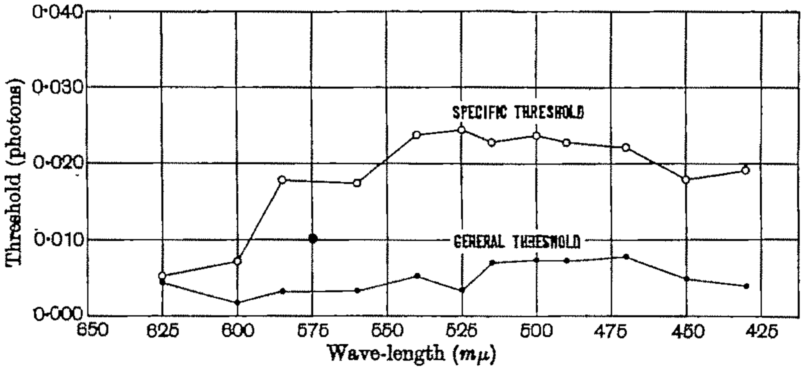

Fig. 2. The general and specific thresholds for the different wave-lengths of the spectrum. The axis of abcissae should be marked 'wave-length ($m\mu$).' The axis of ordinates should be marked 'Threshold (photons).'

Series of observations which were made with smaller field sizes (1·5° and 0·5°) gave higher values for both the specific and the general threshold, but did not indicate any marked change in the relative values of the colourless interval throughout the spectrum.

Let us now consider how the foveal colourless interval is to be theoretically interpreted. To begin with, let us determine whether the scotopic hypothesis proposed by Abney and Watson is adequate to the facts.

In favour of this hypothesis is the fact that the shortest wave-length in the spectrum for which the colourless interval is absent was found to have the value 665$m\mu$. For, according to the data of Hecht and Williams[1], this is almost precisely the longest wave-length of light which will elicit a response from the retinal rods. This makes it seem plausible that the presence of a colourless interval in the fovea may be due to the existence of a small number of rods in that region.

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[1] S. Hecht and R. E. Williams, "The visibility of radiant energy and the absorption spectrum of visual purple," *J. Gen. Physiol.* 1922, v, 1.

Other facts, however, furnish a preponderance of evidence against Abney and Watson's conception. In the first place, according to my observations, the foveal specific threshold is lowered by adaptation to darkness. One would expect adaptation to bring about an augmentation of achromatic scotopic function which would cause an increase, rather than a decrease, in the threshold for colour.

In the second place, I would particularly emphasize the fact that a reduction in the size of the stimulus field raises the specific threshold for the fovea. One can scarcely suppose that the proportion of rods becomes greater the more closely one approaches the centre of the fovea. There would be more reason to expect quite the contrary, if indeed the fovea contains any rods at all.

The problem is complicated, however, by the circumstance that the general threshold also varies with the size of the field. These relationships have been studied in some detail by Abney. In his *Researches in Colour Vision* (p. 155), Abney presents graphs showing the way in which the two thresholds depend upon the diameter of the stimulus, for foveal stimuli ranging between 1° 57′ and 1′ 30″.[1] The wave-lengths employed were 589 and 548$m\mu$. According to these data, both the logarithm of the specific threshold, and the logarithm of the general threshold, are linearly related to the logarithm of the field diameter; *i.e.* the two graphs, plotted in logarithmic terms, are straight lines. These two lines, however, have different slopes; that is to say, the two thresholds obey different laws in respect to the size of the field[2].

If for a given wave-length the two linear graphs, for the specific and general threshold respectively, are sufficiently prolonged, they will intersect. But one is not justified in drawing, as Abney does, the conclusion that "there is an angular aperture subtended on the retina at which any ray will be extinguished both for colour and light at the same time[3]." For, in the case of one wave-length (589$m\mu$), this intersection actually corresponds to a visual angle of 38″, and in the case of the other wave-length (548$m\mu$), to a visual angle of 0·5″. The first angle is so small that a stimulus of that size would fall upon a single retinal cone. The second angle is so very much smaller as to make Abney's inference physiologically meaningless.

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[1] Due to an arithmetical error (which does not affect the accuracy of his graphs), Abney has described the two smallest fields as 9′ 9″ and 3′ 3″. It is clear from other data which he gives that the correct figures are 4° 30′ and 1′ 30″.

[2] The graphs yield the following equations: for the specific threshold, $i=a/d^{1\cdot 11}$, and for the general threshold, $i=b/d^{1\cdot 67}$, where i is the threshold intensity, d is the diameter of the field, and a and b are constants.

[3] P. 180.

Thus there is no reason to suppose that a sufficient diminution of the field diameter will abolish the colourless interval for the whole spectrum. And it is in any case certain that there is a colourless interval for ordinary point stimuli of less than spectral saturation. It is an experience familiar to astronomers that stars which appear coloured when viewed through a telescope of high magnifying power often seem colourless to the naked eye. The telescope of course simply increases the intensity of the starlight without magnifying the optical image.

Finally, it must always be remembered that the complete de-saturation of colours in the neighbourhood of the threshold is merely the limiting stage of the partial de-saturation which occurs at less extreme intensity. It is probable that the same principle which explains the colourless interval will also explain this partial de-saturation. But de-saturation occurs even in the case of red stimuli of wave-length greater than 665$m\mu$, which do not affect the retinal rods. Moreover, the spectral colours attain their greatest saturation only at an intensity which is thousands of times the general threshold. It is quite unlikely that the supposed foveal rods could contribute an appreciable proportion of the visual response to intensities of this order.

We cannot pause to review all the explanations of the colourless interval which have been proposed. Most of them, like that of Abney and Watson, ascribe the phenomenon to some peculiarity in the response of the individual receptors which appears at low intensity. There are reasons for believing that a hypothesis of this type is inadequate.

For example, Hering assumed that the colourless interval is due to the fact that the threshold of the 'white' process is lower than that of any of the chromatic processes. Since, however, the colourless interval is absent in the red, one would have to suppose that the threshold of the 'red' process is as low as that of the 'white' process. It is then difficult to see why there should be a colourless interval in the violet, since, on Hering's principles, the 'red' process is a component of the retinal response underlying the violet sensation.

To escape this difficulty, it may be postulated that colour is discerned only when the *proportion* of chromatic to achromatic excitation exceeds some critical value. But one who makes such a postulate is not explaining the phenomena of colour sensation solely in terms of individual receptor processes; he is introducing a factor of discrimination. The specific threshold would be ultimately dependent on the ability to differentiate between an achromatic excitation which is pure and one which is adulterated with a slight trace of chromatic excitation.

An assumption of this sort was indeed made, in an incidental fashion, by Hering in his early writings. "Sinkt der Bruchtheil, welcher vom Gesammtgewichte einer Empfindung auf eine ihrer Componenten kommt, unter einen gewissen Werth, so sind wir nicht mehr im Stande, diese Componente als solche herauszufühlen....Im Schwarz und Weiss sind eben die gleichzeitig mit empfundenen Farben 'unter der Schwelle,' weil ihr relatives Gewicht zu klein ist[1]."

Helmholtz, in attempting to account for the colourless interval, once expressed himself in a somewhat similar fashion. "Es erklärt sich... dadurch, dass zur Unterscheidung der Helligkeit nur die ganze vorhandene Lichtmenge von absoluter Dunkelheit unterschieden werden muss. Zur Unterscheidung einer kleinen Menge Weiss von einem farbigen Licht müssen dagegen Verhältnisse von Lichtmengen zweier Grundfarben von einander unterschieden werden[2]."

The fact that the specific threshold is determined by the proportion, rather than the absolute amount of chromatic response, is clearly shown by the relatively large foveal colourless interval which is observed in the case of unsaturated colours. Thus Donders and Aubert found a very considerable difference between the general and specific thresholds in the case of pigment colours for foveal vision. A simple experiment will suffice to show the dependence of the colourless interval upon saturation. If a coloured paper is mixed upon a rotating disc with varying amounts of an equally brilliant grey, the reduction of illumination required to extinguish colour will be found to decrease as the grey sector is enlarged.

A smaller colourless interval is naturally to be expected in the case of the colours of the spectrum on account of their greater saturation. But there is no reason to think that any different principle is involved, or that any special hypotheses must be made in order to explain the behaviour of these colours. Moreover, the distribution of specific thresholds in the spectrum (Fig. 2) can be simply accounted for. The more saturated a colour, the lower will be its specific threshold, or, in other words, the same factor that causes a spectral hue to appear saturated at higher intensities makes it retain its saturation as the intensity is made very low.

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[1] E. Hering, *Zur Lehre vom Lichtsinne* (Vienna, 1878), p. 85. I am indebted to Dr E. G. Boring for calling my attention to this passage.

[2] "Kürzeste Linien im Farbensystem," *Zschr. f. Psychol.* 1892, III, 108. See p. 123.

The absence of the colourless interval in the red can now be given a simple interpretation, since the red is distinctly the most saturated colour in the spectrum. In the case of this colour the proportion of chromatic to achromatic component never sinks below the threshold of discrimination.

We have seen that this idea, that colour perception is based upon discrimination, was expressed by Hering and Helmholtz. It has also been stated, and with more explicit recognition of its significance, by Johannes von Kries. In discussing the problem of colour thresholds, this author says:

> One can explain these facts (in the usual fashion of the Hering theory) by assuming that the total value of the stimulus for the colour sensing portion of the visual organ falls below the threshold. But if we conceive the substrate of colour sensation in this manner, as an independent component, then it becomes incomprehensible, and in contradiction with all our other physiological knowledge, that when we vary the stimulating light...*qualitatively,* the threshold value is fixed not by an absolute quantity of coloured light, but by a certain ratio of coloured to white. Since we can scarcely conceive of a peripheral sensory apparatus for which this *ratio* shall determine the threshold, we are forced to adopt the notion that the colour threshold is of a different sort from most of the other sensory thresholds with which we are familiar...[1].

On Hering's principles, we must suppose the colourless interval to be determined by two factors, first, the increased proportion of achromatic process at low intensities, and second, the dependence of colour sensation upon the ratio between chromatic and achromatic process. But there is reason to believe that this first factor need not be assumed, and that a simpler and more likely hypothesis can be found which is based upon the second factor alone.

Namely, it may be that the critical ratio between chromatic and achromatic process[2] which must be surpassed in order that colour shall be discriminated is itself a function of intensity. In other words, a reduction of the intensity of stimulus lowers the discriminative power of the visual mechanism.

It is a well-known fact that low intensity introduces a weakening of other visual discriminatory functions. The ability to discriminate intensities, as well as spatial and temporal differences of stimulation (cf.

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[1] "Die Gesichtsempfindungen," *Nagels Handb. d. Physiol.* (Brunswick, 1905), III, 272. It ought to be said that von Kries has the thresholds of peripheral vision in mind in this passage; but his logic could be applied with equal justice to the foveal colourless interval.

[2] Our reasoning here is not based upon the Hering conception, or any other special conception, of the nature of achromatic process.

the data on visual acuity and critical flicker frequency) is lowered. It is possible that the de-saturation and ultimate extinction of colours may be associated with these other discriminatory defects by way of a common physiological factor.

Moreover, there are other ways, besides reducing the intensity, in which a colour may be extinguished. With a fixed value of intensity, one can force a colour below the specific threshold by diminishing the area of stimulation—just as, with a fixed area, one can produce the same effect by lowering the intensity. Within limits, at any rate, area and intensity act reciprocally.

This suggests that the two factors, area and intensity, may have a basically similar action, in that they both affect the discriminatory mechanism. There is still a third factor which may be compared with these two, namely time. If the time of exposure is too brief, one sees grey rather than a colour; there is a 'temporal colourless interval.' This interval may be reduced by increasing the area of the stimulus[1], or by increasing its intensity[2].

While it is possible to interpret all these relationships in terms of special assumptions regarding the behaviour of the independent achromatic and chromatic processes of the Hering theory, it seems simpler and less arbitrary to regard them in the light of the unitary principle of 'discrimination' which has been proposed here.

It is suggestive that there is a phenomenon analogous to the colourless interval in the domain of auditory sensation. If the loudness of a pure tone is reduced sufficiently, the pitch of the sound becomes indeterminate, and the sensation degenerates into a noise. Also sounds of very low intensity require a larger minimum number of vibrations to produce an experience of pitch.

With these facts in mind, we realize that the disappearance of colour at low intensity is not an isolated phenomenon, but rather a special case of the degeneration of quality under unfavourable conditions. It may be doubted, then, whether we need to postulate special photochemical factors, or indeed any factors which are unique to the visual mechanism, in order to account for the colourless interval.

[1] A. Brückner and R. Kirsch, "Untersuchungen über die Farbenzeitschwelle," *Zschr. f. Sinnesphysiol.* 1912, XLVI, 364.

[2] H. Piéron, "Lois de l'établissement du chroma des impressions lumineuses," *C.R. Acad. Sci.* 1929, CLXXXIX, 194.

IV. THE INTENSITY REQUIRED FOR MAXIMUM SATURATION.

The only quantitative data relating to the maximal saturations of the spectral colours are those of I. A. Haupt[1]. This experimenter found the rather striking result that the spectral yellow (580*mμ*) reaches its greatest saturation at a very high intensity, as compared with the other colours. For wave-lengths on either side of the spectral yellow the intensity corresponding to the maximum sank to a relatively low level, which showed no pronounced variation with wave-length. Of the seven wave-lengths employed, ranging from 655 to 463*mμ*, the wave-length 463*mμ* (blue) was found to reach its maximum saturation at the lowest intensity[2]. Haupt has also measured the number of just noticeable saturation differences which accompany the increase of intensity of a spectral colour from the specific threshold up to the point of maximum saturation. The order of the colours in regard to the number of saturation steps proved to be just the reverse of that obtaining in the intensity measurements, *i.e.* blue had the greatest number of steps and yellow the least.

I have thought it desirable to make a series of determinations of the point of maximum saturation, employing a large number of wave-lengths including the whole spectral range. The method employed was a simple one. The intensity of spectral light was varied by means of the rheostat in series with the spectrometer lamp; beginning with an intensity below the critical point, the intensity was raised fairly rapidly until a point was reached at which the saturation of the colour no longer increased but began to decline. Twenty wave-lengths, varying from 700 to 440*mμ*, were studied, and fifteen observations of the maximum point were made with each wave-length. The field size was 3°.

This kind of observation is exceedingly difficult, and no great accuracy can be claimed for the results. They are probably adequate, however, to give a general characterization of the manner in which the maximum point varies for the different wave-lengths. The data are listed in Table II, and are graphically described in Fig. 3.

The results are in broad agreement with those of Haupt, in that the curve has a maximum in the middle of the spectrum, and drops to low values at the spectral end-regions. From these data we can draw a rough generalization: the more saturated a hue, the lower the intensity at which its saturation becomes a maximum. This rule is not rigorously valid, because the violet reaches its maximum at a lower intensity than the red.

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[1] "The selectiveness of the eye's response to wave-length and its change with change of intensity," *J. Exp. Psychol.* 1922, v, 347.

[2] Determinations were made both with a grey background illumination and without it, the two conditions yielding quite different results. I am quoting only the former series.

Table II.

Wave-length ($m\mu$)	Intensity required for maximum saturation (photons)	Wave-length ($m\mu$)	Intensity required for maximum saturation (photons)
700	42	550	181
660	44	540	193
635	36	525	155
620	57	515	85
600	49	505	62
592	64	490	19
585	134	480	24
575	175	470	15
565	219	460	12
555	182	440	9

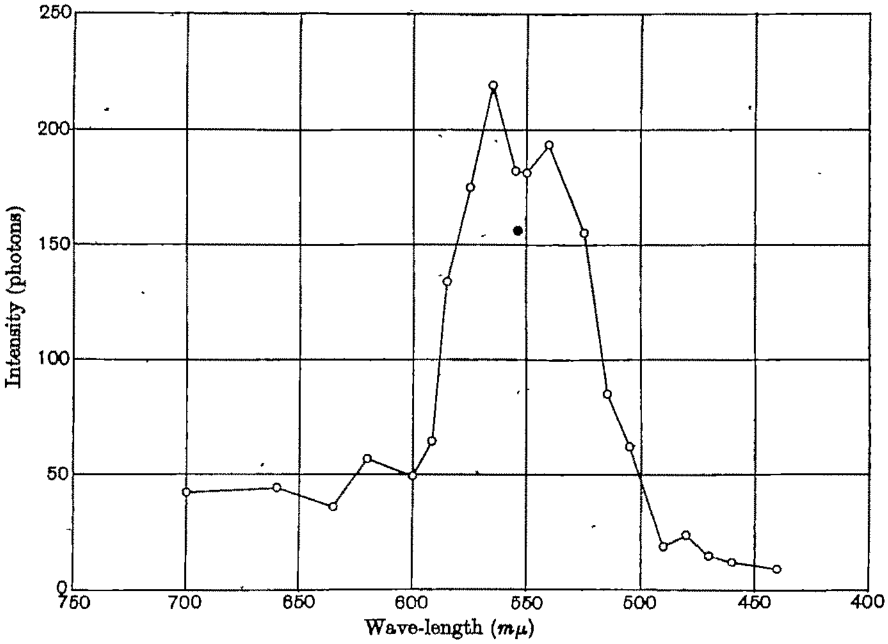

Fig. 3. The intensity required to produce maximum saturation, as a function of wave-length. The axis of abscissae should be marked 'Wave-length ($m\mu$).' The axis of ordinates should be marked 'Intensity (photons).'

The explanation of this relationship is difficult. Following the hypothesis already suggested, we may interpret the diminution of saturation at lower intensities as a direct effect of weak light upon qualitative discrimination. Some other factor is needed to account for the decrease of saturation at high levels of intensity, and thus for the existence of a maximum. (The hypotheses proposed by Hering and Helmholtz may be recalled here.)

It is of interest to note that there is a similarity between the behaviour of colours with respect to intensity increase and with respect to fatigue. H. Sheppard has reported[1] some experiments in which the eye was fatigued with colour for a time sufficient to produce complete desaturation. The time required to destroy the sensation of colour varied in the case of different colours of equal brightness, being least in the violet and blue, somewhat greater in the red and blue-green, and greatest in the yellow. The same ranking appears here as is exhibited in Fig. 3. The colours which lose their saturation first as the intensity is raised are also most quickly extinguished by chromatic fatigue; paradoxically, the highly saturated colours are most susceptible to both influences.

V. THE CHROMATIC THRESHOLD WITH RESPECT TO WHITE.

The next experiments consisted of measurements of the quantity of white light which when added to a spectral colour will suffice to extinguish the colour sensation. In such a measurement one can proceed in either of two ways. The quantity of colour may be held constant, and a variable amount of white added to it; or to a fixed amount of white one may add varying amounts of colour. The latter procedure is more convenient in practice, and was adopted here. Stated in different terms, applying more directly to the actual conditions of observation, the experiments consisted of determinations, for a fixed quantity of white light, of the least addition of coloured light which could be perceived. We may describe this just noticeable addition as the 'chromatic threshold with respect to white.' The ratio between this threshold and the intensity of the white light may be called the 'fractional chromatic threshold.' It was thought that this fractional threshold might be found to vary inversely with the saturation of the colour.

At the time of making these experiments, I was unaware of the recent work of Priest and Brickwedde[2] along similar lines. These investigators have made very precise measurements of the spectral chromatic thresholds, under somewhat different conditions from those involved in my experiments. As yet their complete data have not been published, and the results have appeared only in the form of a brief abstract. There appears, however, to be a degree of agreement between our results which is satisfactory, considering the unlike experimental arrangements, and the cruder technique employed by myself.

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[1] "Foveal adaptation to colour," *Amer. J. Psychol.* 1920, XXI, 34.

[2] I. G. Priest and F. G. Brickwedde, "The minimum perceptible colorimetric purity as a function of dominant wave-length with sunlight as neutral standard," *J. Opt. Soc. Amer.* 1926, XIII, 306.

Apparently Priest and Brickwedde have determined the chromatic thresholds for only a single intensity of the white stimulus. I have followed the variations of the chromatic threshold with intensity as well as with wave-length.

According to G. Révész[1], the chromatic threshold for any given wave-length is a linear function of the intensity of the initial white stimulus. Unfortunately, Révész's measurements are expressed in terms of arbitrary units, which makes it impossible to draw any comparisons between the results for different colours. Hence it seemed desirable to make further measurements of this sort, employing the uniform scale which is furnished by units of retinal illumination.

The following procedure was adopted. The lamp *Z* was used as a source of white light, as in the experiment on the colourless interval. The intensity of stimulation received from it was controlled by moving the ground glass *G*. A centimetre-scale was attached to the framework on which the ground glass moved, so that its distance from the lamp could be read. A calibration of intensity *versus* distance was then carried out. Any desired intensity could then be obtained by setting the ground glass at the distance indicated by the calibration curve.

For the spectral colour under investigation, a calibration curve, showing the relation between intensity and the voltage of the spectrometer lamp, was obtained by the flicker method. Voltmeter readings could then be translated into intensities by means of this curve.

The intensity of white light was first adjusted to some predetermined value. Monochromatic light was then gradually added, its intensity being varied by means of the rheostat in series with the spectrometer lamp, and the point was determined at which a tinge of colour was just visible. After a series of observations of this sort, another series was made in which a supraliminal admixture of spectral light was reduced until the field appeared pure white. Ordinarily, twelve observations were made for each wave-length at each intensity—six of the 'ascending' and six of the 'descending' threshold—although in the case of some of the colours having smaller thresholds twenty-four determinations were secured. The average of all observations for a given wave-length was taken as representative of the threshold. The stimulus field subtended a visual angle of 1·5°.

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[1] "Über die Abhangigkeit der Farbenschwelle von der achromatischen Erregung," *Zschr. f. Sinnesphysiol.* 1907, XLI, 1.

The data are reproduced in Table III. In the rows marked T., I have given the value of the chromatic threshold in photons. Directly under each T. value, in the row marked R., will be found the corresponding ratio between the white stimulus and the threshold chromatic stimulus (*i.e.* the reciprocal of the fractional chromatic threshold). Thus, for the wave-length 660$m\mu$, the chromatic threshold is 4 photons when the white stimulus has the value 200 photons, and the reciprocal threshold is 50. The just perceptible proportion of colour is 1/50.

Table III.

Wave-length ($m\mu$)		Intensity of white (photons)											
		40	70	100	200	300	400	500	600	700	1000	1500	2000
660	T.	—	—	—	4·0	7·0	8 4	11·0	13·0	13·7	—	—	—
	R.	—	—	—	50·0	42·9	47·6	45·5	46·2	51·1	—	—	—
630	T.	—	—	—	7·3	10·8	13·3	15 4	18·0	25·5	—	—	—
	R.	—	—	—	27·4	27·8	30·1	32·5	33·3	27·5	—	—	—
605	T.	—	—	4·0	18·0	24·6	35·8	49·0	65·0	81·0	—	—	—
	R.	—	—	25·0	11·1	12·2	11·1	10·2	9·2	8·6	—	—	—
575	T.	8·0	9·9	12·7	29·0	50·0	92·0	120·0	170·0	230·0	—	—	—
	R.	5 0	7·1	7·9	6·9	6·0	4·3	4·2	3·5	3·0	—	—	—
540	T.	—	—	4·8	16·2	31·2	42·0	60·0	72·0	91·0	—	—	—
	R.	—	—	20·8	12·3	9·6	9·5	8·3	8·2	7·7	—	—	—
505	T.	—	—	—	15·5	25·0	37·3	52·0	60·0	—	—	—	—
	R.	—	—	—	12·9	12·0	10·7	9·6	10·0	—	—	—	—
485	T.	—	—	—	10·4	15·8	20·0	27·8	35·0	55·0	—	—	—
	R.	—	—	—	19·2	19·0	20·0	18·0	17·1	12·7	—	—	—
470	T.	—	—	—	6·4	7·6	10·0	12·6	15·0	18·7	—	—	—
	R.	—	—	—	31·2	39·5	40·0	39·7	40·0	37·4	—	—	—
455	T.	—	—	—	2·3	—	3·3	—	4·6	—	7·1	12·3	16·7
	R.	—	—	—	87·0	—	121·0	—	130·0	—	141·0	122·0	120·0
440	T.	—	—	—	1·5	1·9	2·4	2·8	3·4	4·1	4·6	10·3	16·0
	R.	—	—	—	133·0	158·0	167·0	178·0	176·0	171·0	167·0	146·0	125·0

In Fig. 4, a typical set of data is plotted, corresponding to a white of 500 photons. The reciprocal of the fractional chromatic threshold is represented as a function of wave-length.

This curve yields an interesting and unforeseen relationship. Namely, its form is strikingly similar to that of a curve obtained by R. H. Sinden[1] in his study of complementary colours.

Sinden has determined, for various spectral complementary pairs, the relative intensities of the components required to produce a colourless mixture. He assumes that these intensities are in the inverse ratio to the respective saturations of the two components. By a process of extrapolation he establishes the relative 'saturations' of all the spectral colours in terms of a common unit.

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[1] "Studies based on the spectral complementaries," *J. Opt. Soc. Amer.* 1923, VII, 1123.

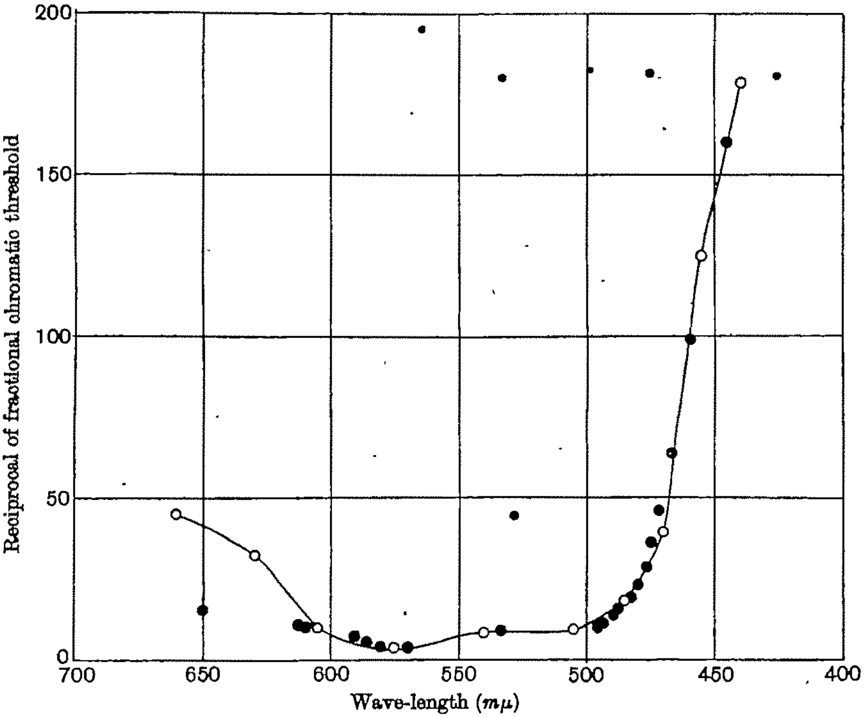

Fig. 4. The reciprocal threshold as a function of wave-length, and its relation to complementation valence. White circles = reciprocal of fractional chromatic threshold. Black circles = complementation valence, according to Sinden. The axis of abscissae should be marked 'Wave-length ($m\mu$).' The axis of ordinates should be marked 'Reciprocal of fractional chromatic threshold.'

In accordance with the remarks made at the beginning of this paper, the present writer would prefer to use the term 'saturation' only in its psychological sense. 'Complementation valence' would be preferable as a description of the quantity determined by Sinden.

To bring out the similarity between the data on chromatic threshold and those on complementation valence, I have multiplied Sinden's measures by an appropriate constant (equal to 4), and have represented them on the same diagram with my own observations (Fig. 4). It will be seen that the agreement is rather good, the only marked discrepancy occurring at the red end of the spectrum[1].

This concordance of measures implies the following principle: The chromatic threshold of a hue is inversely proportional to its complementation valence. In particular, the chromatic thresholds of two complementary hues are in the same ratio as the appropriate intensities required for complementation.

Thus, for example, a violet hue needs but a relatively small intensity in order to cancel the chromatic effect of a greenish yellow and produce a neutral mixture. The same physiological factor governs the size of the chromatic threshold, and is responsible for the fact that only a very small amount of violet need be added to a white in order to produce a perceptible tinge of colour.

The writer has recently learned that Dr Selig Hecht has anticipated him in arriving at this relationship, and has embodied this finding in a paper read before the Optical Society of America, and as yet unpublished. Dr Hecht has shown how the connection between chromatic threshold and complementation valence may be quantitatively predicted on the basis of a new conception of the trichromatic theory which he has worked out, and has verified this prediction by a comparison of the data of Sinden with those of Priest and Brickwedde. This work promises to be of the greatest interest for students of colour vision.

I shall venture to point out a certain difficulty which appears to me to be involved in the interpretation of the chromatic threshold according to Young-Helmholtz principles.

The most reasonable assumption one can make regarding the chromatic threshold is that the quantity of chromatic excitation required to make a colour just perceptible is always the same, no matter what the wave-length[2].

For purposes of illustration, let us apply this assumption to a very simple case. Suppose that a certain wave-length, λ_1, produces an effective[3] chromatic excitation represented by 3 units of red and 1 unit of green. Its complementary wave-length, λ_2, when adjusted to the proper intensity for complementation, will produce 3 units of blue and 2 of green. For the complementary mixture must contain equal amounts of red, green and blue. Now, in this mixture, the total chromatic excitation due to λ_1 is 4 units, whereas the total chromatic excitation due to λ_2 is 5 units.

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[1] The two sets of observations were made under closely comparable conditions. The field size was the same (1·5°), and the threshold chromatic stimuli for the 500-photon white had approximately the same intensities as the corresponding complementary stimuli in Sinden's experiment.

[2] The 'constant quantity hypothesis' is fundamental in Hecht's theory of vision. He has presented cogent evidence in favour of the view that brilliance thresholds (absolute or differential) always involve a constant increment of photochemical change, and it is plausible to assume that a similar principle applies to the chromatic threshold.

[3] *I.e.* neglecting the equal moieties of red, green and blue which cancel one another and form white.

But the respective chromatic thresholds, by hypothesis, correspond to *equal* amounts of chromatic excitation and not to a ratio of 4 : 5. Thus on the Young-Helmholtz theory we should expect the threshold quantities of two complementary lights to bear a different ratio from that in which the two lights must be mixed in order to produce white. Experiment, however, shows that the ratio is the same.

It is possible to reconcile the theory with the facts by making a very special assumption. Namely, it can be assumed that the chromatic threshold is determined not by the total quantity of chromatic excitation, but only by that of the *dominant* excitation. Then, in the case of λ_1, the threshold would be established by the presence of a certain definite quantity of 'red' excitation, and in the case of λ_2, by an *equal* quantity of 'blue' excitation. The subordinate 'green' components would have no influence upon the just noticeable difference. On this hypothesis the threshold excitations of two complementary colours would correspond exactly to the respective excitations required for a neutral mixture.

The hypothesis, however, is arbitrary, and does not seem plausible on its own merits. The facts are interpreted more directly in terms of Hering's theory, or any theory which assumes that equal amounts of chromatic excitation have equal cancelling power. Thus, according to Hering, the wave-length λ_1 will cause a certain concentration change in the red-green substance, and a certain concentration change in the yellow-blue substance. The wave-length λ_2, when adjusted to the proper intensity so that it will neutralize λ_1, must produce concentration changes in the two substances which are precisely equal (and opposite) to those caused by λ_1. On the other hand, according to our 'constant quantity' hypothesis, the amount of change corresponding to the chromatic threshold is the same for all wave-lengths. Thus it follows that the chromatic thresholds of two complementary hues are proportional to their complementation intensities.

Let us next consider the relation of the data to the problem of colour saturation. The curve of Fig. 4 certainly bears some resemblance to the distribution of saturations in the spectrum in that it has its minimum in the yellow, and rises towards the spectral end-regions. In my judgment, however, the correspondence is not complete, the values in the violet being entirely too high as compared with the red. The curve appears to describe some physiological variable which is closely associated with saturation, although not identical with it.

If the chromatic threshold for a given wave-length is plotted as a function of the intensity of the white stimulus, one obtains a curve of the type shown in Figs. 5 and 6. The first of these figures contains the graphs for the longer wave-lengths of the spectrum, and the second contains those for the shorter wave-lengths. The graph for $575m\mu$ appears in both figures, and serves as a standard of reference.

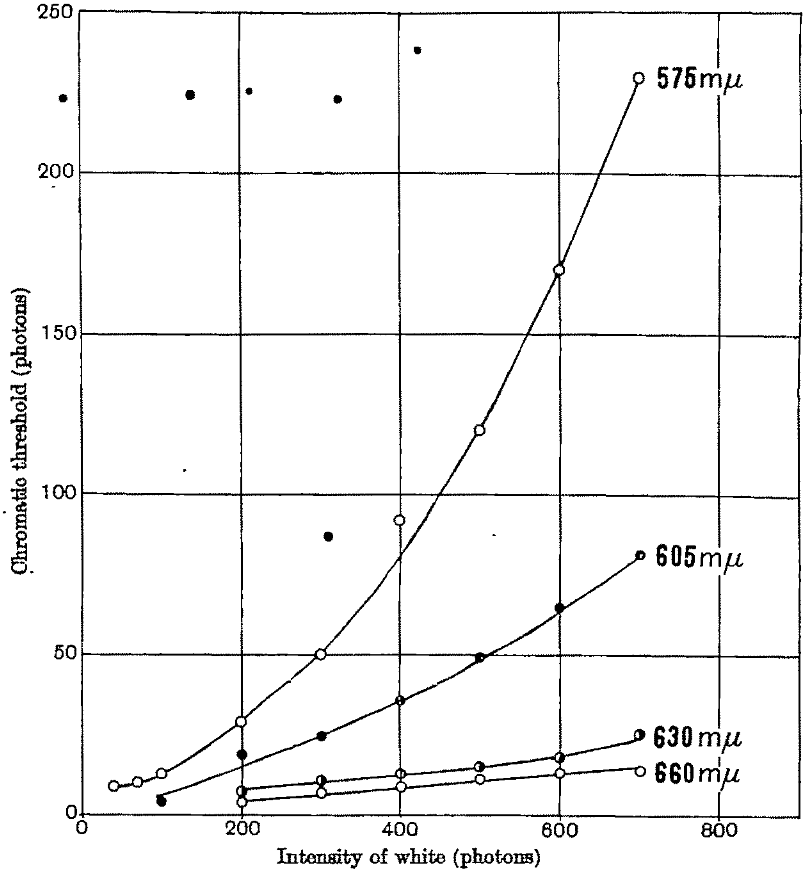

Fig. 5. The chromatic threshold as a function of the intensity of the white stimulus, for the wave-lengths 660–$575m\mu$. The axis of abscissae should be marked 'Intensity of white (photons).' The axis of ordinates should be marked 'Chromatic threshold (photons).'

According to these results, the relationship between chromatic threshold and intensity of white is not strictly linear, as Révész found it. There is a definite upward curvature of the lines, which is most pronounced in the case of the middle wave-lengths. Both the slopes of the lines and their degrees of curvature vary along the spectrum in the same sense that the chromatic thresholds themselves vary, being greatest for the middle wave-lengths, and least for the extreme wave-lengths.

These data may be represented in a different way. As mentioned previously, it was the writer's expectation that the reciprocal of the fractional chromatic threshold might prove to be valid as a perfectly general specification of colour saturation. In our Table III, this reciprocal is given in the rows marked R. Now, let us consider how this reciprocal varies as a function of the intensity of chromatic stimulus, *i.e.* how R. varies with T.

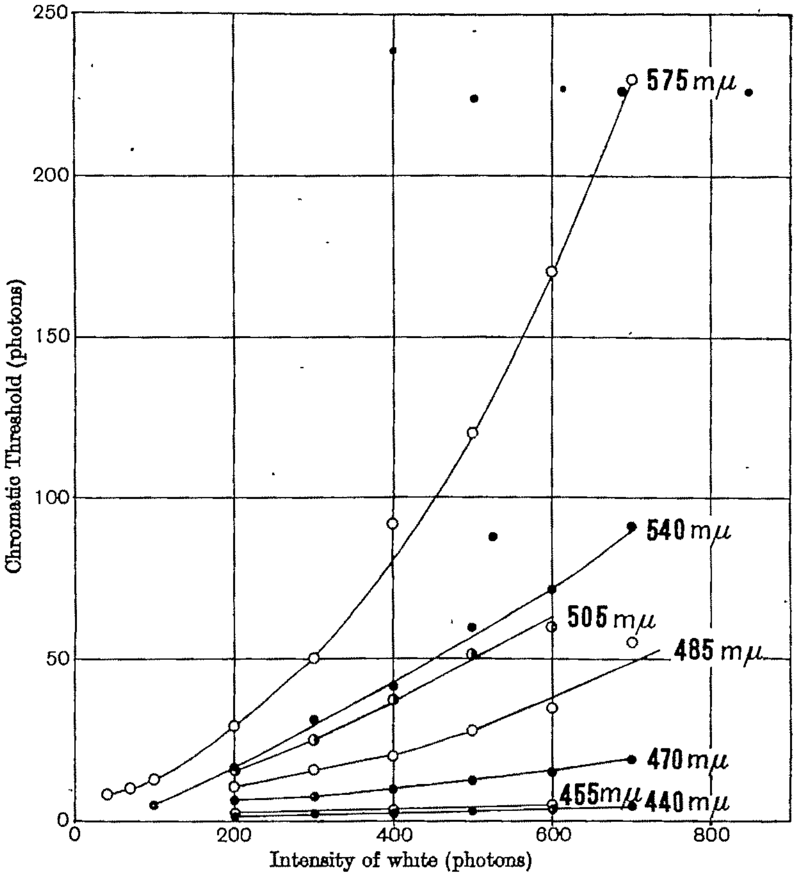

Fig. 6. The chromatic threshold as a function of the intensity of the white stimulus, for the wave-lengths 575–440$m\mu$. The axis of abscissae should be marked 'Intensity of white (photons).' The axis of ordinates should be marked 'Chromatic threshold (photons).'

It will be seen that, while the values are subject to irregular fluctuations, in most cases there is a definite indication of a downward trend at the higher intensities. Also, there is an indication (though less definite) that R. increases with intensity at the outset and rises to a maximum before it decreases. This maximum, however, does not occur at the same intensity level at which the psychological saturation is a maximum. Thus, *e.g.* for the wave-length 575$m\mu$, the reciprocal threshold continually increases between the chromatic stimulus intensities 29 and 230 photons. According to my observations of the actual appearance of this hue (cf. Table II), however, its saturation is constantly rising until an intensity of 175 photons is reached, and only then begins to decline.

We have already noted that the reciprocal threshold does not agree with psychological saturation in respect to its distribution in the spectrum. It now appears that there is a further disagreement between the two, namely in regard to their changes with intensity. Thus we are forced to conclude that the chromatic power or chromatic valence of a colour, as measured by its threshold with respect to white, or by its proportional intensity in a complementary mixture, is not identical with its saturation.

One might propose to measure this chromatic power in a different manner. To a spectral colour of fixed intensity there might be added a quantity of white light sufficient to cause a just noticeable *de-saturation.* This quantity, it would seem, ought to be greater for the more saturated colours. Nevertheless, experiment[1] seems to indicate that the threshold quantity depends simply on the brilliance of the colour and is independent of its saturation; presumably the threshold would be precisely the same for all colours of equal brilliance.

These results, taken in conjunction with my own, point to the necessity of a 'physiological,' rather than a 'psychological' interpretation of the threshold.

VI. SUMMARY.

1. Experiments are described which indicate the presence of a colourless interval in the fovea for spectral stimuli of wave-length less than 665$m\mu$. For greater wave-lengths, in the red end of the spectrum, the interval is absent.

2. The specific threshold is decreased by dark adaptation, but (with the exception just mentioned) does not coincide with the general threshold even after 2 hours in darkness.

3. The value of the general and specific thresholds (in photons) was measured for the different wave-lengths of the spectrum.

4. It is pointed out that the complete de-saturation of colours near

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[1] Cf. Tschermak, *loc. cit.*

the threshold of visibility is only the limiting stage of the partial desaturation which takes place at moderately low intensities. It is suggested that this phenomenon may be most satisfactorily interpreted as the result of the inadequacy of a discriminatory mechanism. The influences which stimulus, area and exposure time, as well as intensity, exert upon the perceptibility of colours, are considered in the light of this unitary principle.

5. Measurements were made, for the different wave-lengths of the spectrum, of the intensity which is required to produce maximum saturation. This intensity is small in the red, rises to a high value in the middle of the spectrum, and sinks to its lowest value in the violet. Roughly speaking, the more saturated a colour, the lower the intensity at which its saturation becomes a maximum.

6. To a given intensity of white light, there was added an amount of spectral light sufficient to produce a just noticeable colour. This threshold amount was found to be greatest in the yellow, and least in the violet, where it is exceedingly small. If we plot a curve representing the reciprocal of the threshold as a function of wave-length, this curve proves to be closely similar to that obtained by R. H. Sinden for the complementation valences of the spectral colours. It follows that the chromatic threshold of a hue is inversely proportional to its complementation valence; the chromatic thresholds of two complementary hues are in the same ratio as the respective intensities of those hues which are required for complementation.

7. This relationship may be readily interpreted in terms of the Hering theory, or any theory which assumes that two chromatic excitations which complement each other must be equal in total amount. Its interpretation in terms of the Young-Helmholtz theory, according to which balanced chromatic excitations are in general unequal, meets with considerable difficulty.

8. If for a given wave-length the chromatic threshold is plotted as a function of the intensity of the white stimulus, an upwardly concave curve is obtained. This result is contrary to that of G. Révész, who found a linear relationship. The slopes and accelerations of these curves are greatest for the middle wave-lengths, and least for the extreme wave-lengths, varying along the spectrum in the same sense that the chromatic thresholds themselves vary.

9. The fractional chromatic threshold is not perfectly correlated with subjective colour saturation, but appears to be closely related to it.

(*Manuscript received* 3 *May*, 1930.)

SOME EXPERIMENTS WITH WOOL TEXTILE TRADE ADVERTISEMENTS[1].

BY HENRY BINNS.

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I. THE JUDGMENT OF SPECIALITY MATERIALS.

THE advertising of articles which can be recognized by name and usually by a fixed price has already been investigated thoroughly by advertising experts. But the selling of articles (*e.g.* wool-tops—combed wool) by specialists having a detailed knowledge of one particular class of material to other specialists whose machinery is adapted to that class, comes under a different category. Both persons involved in the sale thoroughly understand the general possibilities of the material, but the buyer is not aware of the blends of wool which are present in the top. Moreover quality judgment is definitely associated with price—which sometimes fluctuates rapidly—and also with time of delivery, length of credit, etc.

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[1] This paper was read to the psychological section of the British Association for the Advancement of Science, Bristol, 1930.

Within limits, competition revolves around the price of a number of qualities offered by different persons or firms. As wool is sold by auction, and is treated in scouring and combing by one of several methods, there can be no guarantee that a quality bought at one time can be repeated at a later date, though a general standard of quality is recognized by the trade. In advertising such goods—which are typical of most classes found during the processing of wool textiles—the cost of advertising must be included in a competitive market price.

There is no monopoly except in so far as the particular blends in one top may meet the peculiar needs of a buyer and cannot be obtained elsewhere.

The problem is whether it is possible to create such an impression on the mind of a buyer, in an advertisement, as will lead him to buy goods after comparison, at a competitive price, which may be reduced by increased sales or increased by advertising costs.

As the object of advertising is to open up new business the probability is that no direct orders will result, for the quality of the new material must first be judged by sight and touch. The advertisement, if properly laid out, should be an introduction for a direct representative or an agent; it might also result in enquiries for samples by post.

In general advertising such as that of soaps, toffees, motor cars, etc., the technical mind endeavours to influence the natural and untrained judgment. This means that wise, sound, biased and non-suggestible judgments are seeking to influence relatively partial, weak, erratic and suggestible judgments. Thus the tendency is to make use of illustrations which stimulate the instincts and sentiments without drawing too much on reason.

It is unusual for the specialist-to-specialist textile advertisement to attempt more than to influence the mind through vision. Stimulation of the sense of touch, and the appeal to memory are lacking. Reason does not enter largely, and the appeal tends to drift into a mere statement of the barest facts. Displays and illustrations are designed to attract momentary attention, rather than to provide a comprehensive and pleasing effect which may leave a permanent impression.

Since little experimental work has yet been done on the technical side of textile advertising, it seemed probable that some fundamental factors of practical utility might result from an investigation.

II. THE METHODS OF THE INVESTIGATION.

(a) *The test advertisements.*

It was possible to obtain proofs of advertisements which were to appear in a new international wool-trade publication. All advertisements were of the same size, 8 in. × 7 in.; none had been published at the time: all had been 'O.K.'d' by the advertisers.

In consultation with Dr Wynn Jones, forty-five of such advertisements were carefully selected, and divided into groups of fifteen each. Into each of these groups an advertisement with similar distinctive features to two other advertisements was introduced. The three 'sets' therefore contained fifteen companion advertisements.

The average ranking according to merit of the three sets of fifteen advertisements by a large number of persons should give a high correlation if a common factor were present.

(b) *The instructions to the judges.*

Each of the three sets of advertisements was placed in a separate cover together with typed instructions which were as follows:

> Please arrange these fifteen advertisements in your order of preference from the best to the worst.
>
> *Set* 1. *Buyer*: In the order in which you think the advertisements would influence you as a buyer, disregarding the nature of the goods.
>
> *Set* 2. *Seller*: In the order in which you would yourself make use of the advertisements, in the hope of influencing people to buy your goods, disregarding the nature of the goods.
>
> *Set* 3. *Pleasant memory*: In the order in which you think they would create a pleasant memory associated with the name of the firm and the goods advertised, disregarding the nature of the goods.

(c) *The subjects.*

Five persons in each of the following groups were invited to judge: thirty-five persons in all:

(1) Wool merchants.
(2) Worsted spinners.
(3) Advertisers.
(4) Advertising agents.
(5) Advertisement salesmen.
(6) Printers.
(7) Commercial artists.

The average of these thirty-five judgments constituted the criterion or Advertisement Standard; and, for reasons explained later, a Wool Merchants' Standard is also used.

In addition five persons in each of the following constituted control groups.

(8) Lady secretaries.
(9) Non-trade ladies.
(10) Boys, Vth form, 16 years.
(11) Boys, Elementary, 14 years.
(12) Five trials by a lady secretary.

Ten more tests were made by the experimenter in order that changes in judgment, in any one person, on different days and times, might be observed.

(*d*) *The conditions.*

The experiments were conducted throughout under well-controlled conditions, the experimenter alone being present with the subject. All identification marks were in code on the backs of the advertisements. No conversation took place during the proceedings, nor were any results shown to the subject until the close of the tests. Previous experience has shown that it is not advisable to lay down any rules or methods for grading. Whatever method was employed a final careful examination was invariably made by the subjects.

(*e*) *Statistical methods.*

Spearman's Footrule formula is used throughout, in calculating the correlations[1]. The percentage gradings of the advertisements are determined by averaging the placings of the various group judgments, and converting these placings into a scale on which 100 per cent. is the highest and 0 per cent. the lowest. The difference between the highest and lowest figure on such a scale measures the reliability (M.R.) of the group likes and dislikes of the materials used in tests.

III. THE RESULTS AND CONCLUSIONS.

The results may, with advantage, be stated in general, rather than in detailed terms. These must necessarily be tentative and subject to further confirmation.

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[1] The numbers (n) used throughout the tests being the same (fifteen), the P.E., except where otherwise stated, is ±0·19.

(*a*) *Mass judgment.*

It might reasonably be anticipated that there would be a moderate difference between the average gradings of the thirty-five persons accustomed to dealing with advertisements and the control groups. The results are as follows, the number of persons or judgments being in brackets. ('All' includes two series of trials by the experimenter.)

Advertisement (35) and 'all' (70).

Set I	+·96
II	+·94
III	+·85
Average	+·92 ±·19

Advertisement (35) and non-trade (25).

Set I	+·88
II	+·76
III	+·60
Average	+·75 ±·19

Advertisement (35) and the experimenter (10).

	First trials	Second trials
Set I	+·81	+·66
II	+·81	+·73
III	+·50	+·31
Average	+·71 ±·19	+·57 ±·19

These figures indicate that there is a substantial agreement between the various groups. They show also, in a general way, that the fifteen styles of advertisement appeal to the natural judgments of individuals as well as to the acquired judgments gained by advertising experience[1].

It will be noted also that there is most agreement in Set I, less in Set II, with a marked drop in Set III.

(*b*) *Styles of advertisements preferred.*

The wool textile trade is divided up into a large number of specialist sections, which constitute almost separate trades, wool merchants and worsted spinners being two of the general divisions. In view of these conditions, it seemed best to take as an Advertisement Standard the opinion of those groups which contribute to the lay-out of advertisements, including the two main trade advertisers. As the wool merchants were an exceptional selection, these are treated as a Wool Merchants' Standard for comparison.

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[1] In order to compare the gradings of the advertisement and control groups (which in this case included two groups by the experimenter) the whole of the forty-five advertisements were arrayed in the order of their percentage scoring: this resulted in a correlation of +0·79±0·03.

If the Advertisement and Wool Merchants' criteria be taken as standards the following are the styles preferred, these being the average percentage gradings of the three sets of advertisements. The advertisements considered to be the best head the list.

	% grading		
Style	Advertisement	Wool merchants	
H.	(1) 90	(3) 79	In which the suggestion of the personal handling of wool is dominant.
T.	(2) 76	(4) 76	In which handling of wool is suggested through the sense of sight.
E.	(3) 71	(1) 89	Illustrations of engines.
S.	(4) 62	(5) 62	Illustrations of charts and laboratories.
C.	(5) 61	(2) 81	Coloured.
O.	(6) 52	(10) 43	Copy and display by the advertisers.
L.	(7) 46	(8) 48	Illustrations of combing and spinning machinery.
R.	(8) 44	(6) 56	Red and black lettering.
A.	(9) 42	(12) 27	Copy displayed by commercial artists.
Z.	(10) 39	(11) 39	Illustrated but crowded displays.
B.	(11) 38	(9) 44	Black grounds.
M.	(12) 38	(7) 53	Illustrations of mills.
W.	(13) 36	(13) 24	Illustrations of warehouses.
P.	(14) 29	(14) 16	Copy displayed by printers: no illustrations.
U.	(15) 26	(15) 14	Reasoned statements without illustrations.
	M.B. 64	75	

The correlation between the two gradings is + 0·83 ± 0·19. The strength of group opinion is indicated in the advertisement group at 64 per cent. and the wool merchants' group at 75 per cent.—both, judging by experience of other tests, being quite a decisive figure.

Taking the trade as a whole, I think the advertisement grading the better criterion. At the upper end of the scale the difference in the placings between the two groups is probably due to the indecision occurring in Set III, which was noted throughout the tests. The most attractive of the forty-five advertisements was designed for a keen competitor of four of the wool merchant judges, and it may be suspected that this and other advertisements of H. and T. styles were unconsciously placed a little lower on this account than E. and C. styles.

Photographs of men handling wool, suggesting the active co-operation of the senses of sight and touch with judgment appear, according to the Advertisement Standard, to be most attractive. The next most desirable are those in which there are illustrations of wool only, without men, personal handling being suggested indirectly through the sense of sight.

An examination of the different styles of lay-out shows in some measure at the bottom of the scale what it is desirable to avoid. The middle area suggests rather featureless characteristics which might be made more individual and striking.

(*c*) *Individual advertisements preferred.*

The following description of the advertisements receiving the five highest and the five lowest scores of the forty-five advertisements indicates the preferences of the thirty-five Advertisement Standard judges acquainted with the needs of the trade.

Style	Score (%)	
H.	83	This advertisement was drafted by the experimenter as a test of psychological appeal. In bold type was the wording: "The velvety touch in Merino tops is absolutely essential if you wish to produce soft handling yarns." A photograph of one of the principals of the firm 'drawing' a 'top' (combed wool) from the ball, as in judging the material, occupied the centre of the page.
T.	81	The chief feature was a photograph of a chart of fifteen standard qualities of tops made by the firm, prepared and certified by the City of Bradford Conditioning House. Much care was taken in the preparation of the card on which the fineness and length of wool fibres are displayed. The seal of the Conditioning House added to the attraction.
H.	81	"London, the world's market for wool" headed a photograph of an aisle of bales, cut open, in the show-room, with three buyers looking at and handling the raw wool in course of their estimation of quality, 'yield' (clear wool left after the removal of grease and dirt) and price.
H.	78	A bold heading of the name and address of a well-known Australian firm of wool-brokers appeared over a large photograph of "a corner of our Freemantle Showroom." This showed an immense shed with wool clearly displayed, particularly in the foreground and with a few persons in the background.
L.	72	The name and address of the firm headed a photograph of "an Improved Noble Comb." The machine was combing scoured wool, the 'tops' (combed wool) being prominently shown.
U.	31	Four-fifths of the space was closely filled with a statistical statement of wool production in one of our colonies. The heading consisted of the name of the colony and "Wool and Mohair." The only illustration was a crest of the colony.
P.	31	This merely contained name and address, "Clothing Wools," Importers and Exporters of a variety of wools and wastes, without illustration or fancy border.
Z.	27	A crowded display with black ground blocks lettered in white: "Good Packing Pays" and "Textile Goods made up for all Markets." These were surrounded by twelve lots of very small type descriptions in five languages.
L.	26	Two small photographs of spinning frames were inserted near the top of the page and below the name. The remainder of the space was filled in by small type descriptions of frames in three languages.
U.	25	An advertisement, in mixed type, with unbroken straight lines on the border and text.

(*d*) *Group differences.*

Two measures are available for comparing the variations between group judgments: (*a*) the percentage differences between the best and worst advertisements (M.R.); (*b*) the averages of ten inter-correlations of the judgments of five persons. These are as follow:

	M.R. (%)	Correlation
Trade:		
Wool merchants	75	+·41
Worsted spinners	81	+·34
Advertisers	80	+·30
Advertising agents	70	+·27
Advertising salesmen ...	68	+·25
Printers	76	+·39
Commercial artists	77	+·59
Non-trade:		
Lady secretaries	71	+·37
Non-trade ladies	65	+·23
Boys, Vth form	52	+·36
Boys, Elementary (14 years)	73	+·48

The percentage figures (M.R.) show that, with the exception of a group of Vth form boys at 52 per cent., there are well-developed likes and dislikes in groups on all the three sets.

The correlations are low and show that, between the strong likes and dislikes, there is considerable variation in the middle area. It is significant that commercial artists have the highest correlation at + 0·59, though (as will be seen later) they are in agreement with Advertisement Standard at + 0·44, the highest being + 0·54. This is no doubt due to the fact that the artist has a strong tendency to select by artistic merit only, ignoring other factors.

(*e*) *Group correlations with Advertisement and Wool Merchants' Standards.*

The averages of five judgments of each of the three sets of advertisements compare with the (1) Advertisement Standard; (2) Wool Merchants' Standard as follows:

	Advertisement Standard	Wool Merchants' Standard
Experimenter: First series	+·72	+·60
Second series	+·56	+·37
Wool merchants	+·54	—
Advertisement salesmen ...	+·48	+·81
Advertising agents	+·48	+·56
Advertisers	+·45	+·51
Commercial artists	+·44	+·16
Worsted spinners	+·43	+·76
Printers	+·42	+·40
Lady secretary—1st series	+·41	+·38
Lady secretaries	+·40	+·61
Boys, Elementary (14 years)	+·34	+·62
„ Vth form	+·35	+·60
Non-trade ladies	+·27	+·35

It would be expected that the repeated judgments of any one person would be more constant than the same number given by different people. But it would not be anticipated that a considerable variation might exist between those of, say, the experimenter, first series at + 0·72 and second series at + 0·56.

(*f*) *Common factors in judgment.*

When the methods of the investigation were planned it was anticipated that, even though the instructions given to judges were varied in each of the three sets, there would be a high correlation between them. The results of the averages of the three inter-correlations between Sets I and II, I and III, and II and III are as follows. The figures in brackets show the ranking of each group.

Advertisement Standard ...		+ ·61
Trade:		
Wool merchants (Standard)	(4)	+ ·54
Worsted spinners	(9)	+ ·44
Advertising agents	(11)	+ ·33
Advertisers	(10)	+ ·40
Advertising salesmen ...	(5)	+ ·50
Printers	(1)	+ ·75
Commercial artists	(2)	+ ·62
Non-trade:		
Lady secretaries	(8)	+ ·45
Non-trade ladies	(7)	+ ·46
Boys, Vth form	(6)	+ ·48
„ Elementary (14 years)	(3)	+ ·58
Experimenter: First series ...	—	+ ·59
Second series	—	+ ·71
Lady secretary	—	+ ·53

Printers and commercial artists show most uniformity, due to specialised experience. Advertisers and advertising agents, on the other hand, give consideration to a great many factors which result in a variety of opinion and independence of thought. It would be expected that a higher correlation might arise in the case of the experimenter. The fact is, however, that whilst he shows well-established uniformity of preference, there is still a margin left for likes and dislikes of particular displays within that margin from time to time.

(*g*) *Repeated judgments.*

Repetition of tests by the same person affords a good indication of the reliability of his judgment and also shows his measured opinion as compared with that of others, *e.g.* Advertisement or Wool Merchants' Standard.

Two series of experiments, with an interval of six months, were undertaken in order to trace variations at different periods. There was an interval of from a day to a week between each test, so that recall of past judgments should not unduly weigh in the decisions.

The following are the correlations with the Advertisement Standard for the two series:

		First series.		
Sets ...	I	II	III	Average
1st trial	·70	·78	·66	·71
2nd „	·78	·78	·50	·69
3rd „	·66	·81	·62	·69
4th „	·78	·83	·57	·73
5th „	·81	·76	·54	·80
Average	+·75	+·79	+·58	+·72
		Second series.		
1st trial	·66	·73	·36	·58
2nd „	·66	·85	·32	·61
3rd „	·54	·70	·31	·52
4th „	·57	·83	·32	·57
5th „	·60	·81	·23	·55
Average	+·61	+·74	+·31	+·56

It will be noted that there is less agreement in the second than in the first series. The day-to-week differences in judgment are not marked. There is, however, considerable variation between the final averages of the three sets. The highest records of Set II indicate that the experimenter agrees strongly with the Advertisement Standard, but there is a much lower agreement on Set III.

If the Advertisement Standard be disregarded and only the uniformity of the various judgments be considered, then there is a striking agreement between the tests. This might be said to be the reliability of the measured opinion of the experimenter if considered in relation to his gradings of the advertisements.

This reliability may be stated as under:

	First series	Second series
Set I	+·97	+·93
II	+·94	+·93
III	+·93	+·90
Average	+·95	+·92

As each of these represents the average of ten inter-correlations, the reliability of the judgments may be said to be remarkably high. In other words, there is a very well-defined opinion on the grading of forty-five advertisements which agrees with the Advertisement Standard in the first series at + 0·72 and the second at + 0·56. There is, however, a shift of preference on the whole between experiments made six months apart.

Repeated judgments by a lady secretary, compared with the Advertisement Standard, are as follow:

Sets ...	I	II	III	Average
1st trial	·46	·60	·08	·38
2nd „	·46	·39	·31	·39
3rd „	·54	·54	·36	·48
4th „	·57	·46	·35	·46
5th „	·54	·46	·08	·36
Average	+·51	+·49	+·24	+·41

This shows a low correlation with the Advertisement Standard: Set III being again the weakest. There is not much day-to-week change.

The inter-correlation between each of the five judgments on the three sets is as follows:

Set I	+·81
II	+·88
III	+·85
Average	+·85

The judgments are uniform, but her measured opinion has little in common with the Advertisement Standard.

IV. GENERAL OBSERVATIONS ON COMMERCIAL TESTS.

(*a*) *Fundamental factors in sound judgment.*

The wool textile trade offers an ideal field for the investigation of 'judgment' in industry, because its raw material has approximately 2000 classes, the factors involved in estimating quality and value being numerous, complex, irregular and fluctuating. Tests of any kind never cover the whole of the existing facts: the judgment of experienced men, however, must attempt to do so. It is, therefore, worth while to attempt an analysis of essential features in judgment so that all those who are practically interested should know with what factors they have to operate.

(*b*) *Key factors in the judgment of wool products.*

The 'key' factors in the judgment of wool products are the criteria of fineness, softness and shortness of wool fibres. The judgments of intelligent boys in the Vth and VIth forms in standard qualities of wool-tops show that, on the average of five trials on each of these three 'key' factors, there is a close agreement with the judgments of men of long experience. The essential feature is that there must be the opportunity for comparison of qualities which must in themselves be capable of definition, *e.g.* qualities must not be blended to make mixed or complex forms of material.

(*c*) *Preference factors.*

Preferential requirements to meet either a special price, market, machinery, or individual taste within a fashion are essentially associated with the judgments of the most experienced men. Specialists in judgment industries must be able to discriminate between samples which are variations of approximately the same quality. In the case of wool this is determined (in addition to the key factors of fineness, softness and shortness) by soundness, uniformity, colour and lustre, crimpiness, character, spinning property and other items. This demands knowledge and experience, and for the lack of it no amount of theory can compensate. Memory standards are established against which single samples may be compared, the most minute differences being detected without direct comparison. Intensive experience creates an unconscious bias in favour of materials usually handled. This bias is a common feature in wool investigation.

(*d*) *Combination of factors.*

In buying and selling (which includes advertising) any or all of the numerous factors involved in sound judgment *may* be present. There is always an unknown element in competition, one seller not knowing the conditions or prices of other sellers: the buyer rarely discloses his hand. This tends to develop a type of imagination demanding estimation, speculation and some risk.

The more readily an individual can combine the various factors involved, the better will be his judgment.

The balance of thirty-five specialist judgments appears to give a grading of all-round value, relatively free from the bias of group or person. The repeated judgments of the experimenter, which are fairly constant with themselves, point to the possibility of one person being able to cover the essential points, free from bias, if he is sufficiently acquainted with the factors indicated above.

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VIERORDT'S LAW.

By K. C. MUKHERJEE.

(*From the Psychological Laboratory of the University of Dacca.*)

According to Vierordt the space limen of a point in a limb varies inversely to its distance from the axis of its rotation. He laid it down as a general rule on the basis of his experiments in which he observed continuous decrease of space limens from the shoulder to the tips of the fingers of his subjects.

Spatial discrimination is no doubt maximal at the tip of the finger, but certain variations from the general rule appear to occur.

The palmary side of the right-hand limb was chosen for exploration. A chalk line from the shoulder to the tip of the middle finger was drawn. The middle finger, the palm, the forearm and the arm were each divided longitudinally into three equal parts, *A*, *B* and *C*. Each of the *A*, *B* and *C* parts of the middle finger and the palm was marked into small areas *a*, *b* and *c*, and each of the *A*, *B*, *C* parts of the forearm and the arm was marked into small areas *a*, *b*, *c*, *d*, *e*, *f*. Thus we obtained a series of points *a*, *b*, *c*; *a*, *b*, *c*;...*a*, *b*, *c*, *d*, *e*, *f*; *a*, *b*, *c*, *d*, *e*, *f*, along the line drawn from the tip of the finger to the top of the arm.

All necessary materials such as adjustable screen, felt, Spearman's aesthesiometer, etc., were used and adequate precaution taken in the experiment, which lasted several months.

Observations of the space limens of different points of the middle finger show that the tip of the finger yields to the smallest space limen in the series. This is in agreement with the law, but certain irregularities are found when the space limens of the points adjacent to joints are compared to those of the other points lying more distant from them. The points adjacent to joints are more delicately discriminative than the points in the middle part of the region. In the *A* part of the middle finger the space limen of *b* is greater than that of *a* or of *c* in five subjects, greater than *a* but less than *c* in two subjects, and greater than *a* but equal to *c* in two subjects. Even in the two cases where the liminal value of *b* is less than *c* the difference slightly varies between 0·1 and 0·2 mm., whereas in five cases the liminal value of *b* is clearly greater than that of *c*, and in the remaining two cases they are equal to each other. Thus the liminal value of *b*, when compared with that of *c*, does not conform to the Vierordt's law. In the *B* part there is not much deviation from what is found in the *A* part of the middle finger, but in its *C* part the space limens are in line with the law. In the palm of the hand the mean limen of the *B* part is greater than that of *A* or of *C* parts except in the cases of two subjects. Thus the sensitivity of the middle part of the palm is less than that of the part lying longitudinally on either side of it. The recorded limens of the forearm show a markedly small sensitivity of its middle parts though a slight divergence occurs in the case of two subjects. This relative deficiency of sensitivity of the middle parts, as compared with that of the upper and lower parts terminating in the joints on both sides, is very convincingly found in the case of the arm in which the upper part towards the shoulder proves most sensitive. Thus a general survey of the records shows that the tip of the middle finger is most sensitive here, but the other extreme part at the shoulder which should, according to the Vierordt's law, be the dullest, displays on the contrary a fairly delicate sensitivity. Over the remaining parts of the arm the general trend of sensitivity is in agreement with the law except that the areas adjacent to joints are more sensitive than the areas lying between two joints.

This result agrees with general considerations relating to the positive correlation of the mobility of points with delicacy of spatial discrimination, for the areas adjacent to joints more capable of the former are more free to exploratory movement than those which lie farther away.

It is not easy to say what are the physiological peculiarities corresponding to this difference in the keenness of sensitivity in different areas along the arm. Apparently psychological factors such as exercise, and attention, and the general mobility of the parts concerned which no doubt contribute to the development of tactile discrimination do not give a complete explanation of the facts. The highly developed power of localisation in the blind and the delicacy of tactile discrimination at the finger-tip of compositors prove that tactile perception is improved by practice. But many conditions, such as compression of the veins, the anaemic state of a raised limb, the action of cold, strychnine, etc., alter the delicacy of tactile discrimination and sometimes render it altogether blunted. Kulpe attributes the difference in spatial discrimination of different touch areas mainly to physiological peculiarities of peripheral excitation, but the characteristic nature of the peripheral peculiarities he has not made clear. Weber has also emphasised the importance of the

PUBLICATIONS RECENTLY RECEIVED.

Intellectual Growth in Young Children. By SUSAN ISAACS. London: George Routledge & Sons, Ltd. 1930. Pp. ix + 370. 12*s.* 6*d.* net.

Books about children concern themselves with a topic which is admittedly of wide human interest and importance, and which therefore from the nature of things should appeal to a very catholic audience. That they will in any case be read is hardly an adequate excuse for the cumbersome form they often take, but rather the contrary. When the method is to present information in the form of selections from an original collection of records of children's conversations or behaviour, accompanying or preceding these by a critical and explanatory commentary or analysis, the most important part of the total contribution, in the case of many of them, lies in the scattered mass of recorded original material. That is the pity of it! The kind of experimental technique which it is necessary to use—observation and diligent recording of innumerable individual reactions, valuable or inconsequential—seems almost inevitably to lead to prolixity in the presentation of the material. Need it inevitably be accompanied by prolixity in the statement of general conclusions? The result so often is that appreciation of the true value of such a contribution to knowledge is restricted to the specialist or enthusiast, who alone will be prepared to devote to the collected data a not inconsiderable amount of attention and discrimination.

The present book does not altogether escape some of these criticisms, in spite of the high level attained in its frequently illuminating analyses and discussion. It is the first volume of a proposed trilogy, the object of which is to make an exhaustive study of the group behaviour of young children. It is based upon a three years' observation made on children between the ages of 2 and 10 years at the Malting House School, Cambridge. The present volume is concerned with the intellectual aspects of their mental development: a second volume is promised shortly on Social Development, and the trilogy is to be completed by a third volume of Individual Histories, which is to describe as a whole the development of certain selected children during the three years of the records. The first volume of 370 closely printed pages is therefore to be regarded only as an instalment of what the author has to say.

A preliminary chapter examines critically the value of qualitative records of children's behaviour, and outlines the methods by which records were obtained at the Malting House School. The subtle difficulties involved in an adult's attempt to observe and to interpret 'free' or unfettered childish life are clearly grasped and explained. It is frankly admitted that the presence of any adult observer in their midst will tend to affect unduly the course of events, and so militate against the chances of his ever being able to observe children's activities under really free and natural conditions. It is claimed however that the conditions which existed at the Malting House School did provide for the group of children under observation an environment almost as free as it would ever be possible to obtain.

A chapter of 23 pages describes what are called 'the conditions of observation.' A detailed description is given of the school environment, of the psychological setting of the experiment, and of the educational aims of the experimenters. Briefly, these aims were to stimulate the active enquiry of the children themselves rather than to 'teach' them, and to bring within their immediate experience every range of fact to which their interests reached out. Except in so far as their freedom was limited by the necessities of a minimal framework of routine, of bodily safety and hygiene, no restriction was placed upon the children's activities. But physical surroundings in the schoolroom and garden were so arranged as to stimulate them to active and constructive pursuits, and were modified in detail to meet changing needs as they grew older. The mass of incidents which constitute the cognitive experimental material is grouped, in the book, into three main classes according as it represents the application of knowledge already possessed to new situations, the direct increase of knowledge by experiment and observation, and the social interchange of knowledge in cognitive intercourse—this latter including the 'why's' and 'because's,' and the discussions with corrections and self-corrections. The general modes of thought of the children are concluded to be not fundamentally different from our own.

There is much criticism of Piaget's invocation of 'maturation' (in the sense of the changing 'structure' of the child's mind), to explain certain phenomena which may be rather a function of experience. The author claims that her records suggest that certain interests and certain ways of handling experience, which Piaget considers to belong to a more mature stage of growth, are actually found in much younger children under suitable conditions and became especially frequent under the particular conditions of her own group. Unlike Piaget who holds that the key to intellectual development lies in the first appearance of 'the social instincts' at about seven or eight years of age, she finds much earlier evidence of the influence of direct contact with the external physical world. Both threads must be followed in unravelling the story of development. Even in the earliest years (2–5) her children showed a strong and spontaneous interest in things and events of the physical world, and the idea of mechanical causality was soon evident in their remarks and behaviour. The extent to which these interests appear and are sustained, must be in large part a function of the environment, and of the response of influential adults, and they may easily be suppressed under the conditions of ordinary school curricula.

Evidence is adduced to show that little children do possess the ability to reason both accurately and clearly. Piaget explained the difference between the younger child and the older in this respect, as due to the fact either that the former does not reason at all, or only reasons in the form of perceptual judgment or practical manipulation. Mrs Isaacs holds that, in the younger child, verbal reasoning and the clear formulation of judgments are present, although they spring out of a practical and personal situation, are not sustained and die down again. Other striking differences between the behaviour of the Malting House School group and the children studied by Piaget are considered, and discussed in this section. The author does, however, point out that different levels of functioning can co-exist in the same child, and supplies instances which show that her children sometimes used the modes of thought that Piaget defined as ego-centric and syncretistic. But so do adults in certain circumstances. She ascribes the main dissimilarities between Piaget's data and picture of mental development and her own, partly to differences in the mental ratios of the two sets of children, and partly to the very considerable differences in their opportunity and setting, but largely to his very different method of enquiry. Piaget's 'clinical method' is criticized and qualified at length. The final conclusion is that intellectual growth certainly shows an elastic and living psychological coherence which "is most fully expressed in the continuity of development in noetic synthesis, and in the way in which the later and more highly integrated forms draw their life from the simpler and earlier." The chapter closes with a discussion of the relation between thought and phantasy, and of the bearing which progressive differences in interest may have in determining the characteristic modes of knowing and reasoning at different ages. The importance of the *direction* of concrete interests in this connection is stressed. Quotations lend support to all these points, which find further illustration in the full records supplied in the following chapter (47 pp.).

Light is thrown upon the rival educational values of nature study and zoology by the discussion and records concerned with children's biological interests (55 pp.). These suggest very strongly that, in spite of a fairly general opinion to the contrary, children between the ages of 4 and 10 are more actively and spontaneously interested in animals than in plants, with their lack of movement and sound and their generally slower and less arresting tempo. Everyday interest in the processes of life among their pets, 'looking inside' the dead ones, and following up any offered references to the human body, appeared in time actually to achieve an observational attitude towards these things and concurrently to the complicated problems of birth, growth, sex and death.

For the rest, the book contains an interesting account of four sample weeks in the life of the school (43 pp.), a classified summary of all the activities and pursuits of the children at the school (32 pp.), and finally a series of appendices. The longest and most important of these (by Nathan Isaacs, 58 pp.), is a theoretical analysis of the origin, development and function of children's 'why' questions. Contrary to Piaget, the findings support the popular belief that in these originally sporadic, casual and mild outbursts of childish interest, lies the germ of all later scientific enquiry and indeed of all systematic investigation. The attempt is made to show Piaget's view as a picture of the habitual level of functioning of the child's mind at any one time, complementary to the writer's own more general picture of the functioning of a process of progressive challenging.

While, therefore, this book will be of interest to the educationalist because it contains so provocative a picture of the school life of a group of highly intelligent little children, the psychologist will find in it a useful if rather discursive presentation of the arguments against Piaget's structural view of mental development, and in favour of the more functional or experiential view: a discussion in which the impartially minded may sometimes hear faint echoes of an even more ancient controversy—hereditary versus environmental influences—lifted here to a psychological plane.

M. D. S.

The Mental Development of the Child. By KARL BÜHLER. Translated from the fifth German edition by OSCAR OESER. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. 170. 8*s*. 6*d*. net.

The aim of this book, which is primarily intended for students and teachers, is to present in a relatively brief form a comprehensive account of modern psychological theories regarding the mental development of the child.

It begins with a consideration of the biological foundations of child psychology, about a fifth of the book being occupied with a discussion of such general considerations as the meaning of instinct, training and intellect, and the inheritance of intellectual qualities. The aims and methods of child psychology, and the physical development of a child are other topics with which this section deals, not infrequent reference being made to the importance of Köhler's work with his apes in throwing light upon various of these problems.

A whole chapter is devoted to the child in his first year of life, and includes an examination of the sources and development of language and of the nature of the infant's conscious states. The bearing of the Gestalt point of view on the last problem is particularly interesting.

In the other chapters the author considers the memory and imagination of children, summarizes the literature dealing with the topic of their drawings, outlines the evolution of childish thinking and concludes with a discussion of social behaviour.

The book is well illustrated with diagrams and photographs, and its profuse annotations will be of special value to readers who may desire to pursue further any of the points with which he deals. In his treatment of the problems under review Prof. Bühler's attitude is not that of a partisan bent on establishing the legitimacy of one theory at the expense of all others, but rather that of a synthesizer using the concept of development as a central pivot. Although, therefore, the manner of the book is a little disappointing because it is less stimulating and less lively than its subject-matter deserves, it is very well worth reading on account of its careful, painstaking examination of all the evidence at present available.

Eidetic Imagery and Typological Methods of Investigation. By E. R. JAENSCH. Translated by OSCAR OESER. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. 136. 7*s*. 6*d*. net.

This book consists mainly of two essays, the first dealing with eidetic imagery and the two types of eidetics, the basedowid (B) or hyperthyroid, and the tetanic (T) or hypothyroid; the first characterized by the resemblance of its eidetic images to true memory images, and the second by their resemblance to after-images. The second essay gives an account, which is clear but too sketchy, of the two main perceptual and temperamental types, the 'integrate' and the 'disintegrate,' and their relation to the eidetic. The integrate type, particularly common in childhood, is characterized by the close integration and the plasticity of all its mental functions in responding to any situation; the disintegrate, representing in general a higher level of mental development, responds in an analytic and 'piece-meal' fashion. It appears that the B-type of eidetic is an extreme case of the integrate type; the T-type seems to be pathological, though this is denied in the first essay. It is unfortunate for the English reader who is not acquainted with the experimental researches of Jaensch and his school that the experimental bases of these theories are not fully described, but only referred to rather perfunctorily. Thus the reader finds it difficult to comprehend the full implication of the theories or to assess their validity. The relationship of eidetic imagery, on the one hand, to the retinal phenomena of after-images, and, on the other, to the purely central phenomena of memory images, is glossed over too lightly, seeing that it forms the keystone of the theory of eidetics. Nor does there appear to be nearly enough evidence for the hypothesis that all individuals either hereditarily or during their own lifetime must pass through the integrate stage before they can reach the disintegrate. There is little doubt that the theories of Jaensch open up great possibilities for the future; but we cannot as yet determine their validity.

The Basis of Sensation: The Action of the Sense Organs. By E. D. ADRIAN, F.R.S. London: Christophers. 1928. Pp. 122. 7*s.* 6*d.* net.

This little book is an extremely clear and valuable résumé of the work on the impulses in sensory nerve fibres which has been carried out in recent years by Dr Adrian and his collaborators. A brief account of sensory nerves and of the methods of recording nerve impulses is followed by a comparison of the sensory discharges from various receptor end organs. The relations between the intensity of the stimulus, the adaptation of the sense organ, and the frequency of the resulting impulses are described, and Dr Adrian has much that is interesting to say about the functional significance of the different rates of adaptation of the different end organs. The last chapter discusses very briefly the relation between the impulses reaching the brain and the resulting sensation, and the conclusion is reached that the intensity of the sensation is probably proportional to the frequency of the impulses. But the quantitative evidence given here is not very convincing and one might wish for a much fuller treatment of this topic.

The general nature of Dr Adrian's work is, of course, well known to psychologists as well as to physiologists. This brief summary of these researches should be in the hands of all those interested in the psychology or physiology of the senses.

The Laws of Feeling. By F. PAULHAN. Translated by C. K. OGDEN. International Library of Psychology, Philosophy, and Scientific Method. London: Kegan Paul. 1930. Pp. xiv + 213. 10*s.* 6*d.* net.

Paulhan's "Les Phénomènes Affectifs, et les Lois de leur Apparition" appeared in the *Revue Philosophique* in 1884, and was enlarged and published in book form in 1887. Paulhan's work has had an important influence on the development of French psychology, but it has remained comparatively unknown in other countries. Mr Ogden has now translated it into English for the first time, and has included in this volume a translation of Paulhan's essay on "Feeling, Intelligence, and Will in General Psychology."

Paulhan's main thesis is that affective phenomena arise when a tendency is arrested, and psychical energy is thereby liberated and manifested in a relatively unadaptive way. The affective responses tend to appear suddenly, to lack co-ordination, and to monopolize the whole field of consciousness. (The relation of this theory to more recent views, such as those of Rivers and MacCurdy, need not be pointed out.) The feelings are classified in an interesting way according to the strength, persistence, complexity and degree of co-ordination of the arrested tendencies and the affective responses.

Paulhan's work is characterized by a great breadth of outlook, and a striking sense of proportion, and one can find in it many suggestions towards theories which have since been developed in much greater detail by other writers. The consideration of how far his general theory and classification of the feelings are of value would involve a discussion of recent experimental and clinical work on the emotions, which cannot be attempted here. The book is more likely to be of value to psychologists who are looking for suggestions towards a new theory than to students who are looking for an account of what has already been done.

Psychopathology: its development and its place in medicine. By BERNARD HART. 2nd edition. Cambridge University Press. 1929. Pp. 173. 8*s*. 6*d*. net.

These lectures and papers so judiciously conceived and so attractively written were first published in book form in 1927, and are now in their second edition.

Psychology in claiming to be a science has also claimed its right to a place in any consideration of the causation and treatment of disease. Dr Hart, working on the strictly scientific basis of determining the extent to which a proposed concept can be confirmed and verified by the facts, aims to examine and evaluate the degree to which medicine as an established science is justified in accepting as valid these claims on the part of psychology. And one of the most attractive things about the book is the spirit of almost judicial impartiality in which despite, or perhaps because of, the importance of the issues at stake, this aim is accomplished. It would be difficult to find in so compact a form any more lucid or more just assessment of the relative therapeutic values and limitations of such well-known psychopathological tenets or concepts as hysteria and hypnotism, suggestion and persuasion, the idea of 'purposiveness,' dissociation, Freudian analytical theories and methods, or the later contributions of Jung and of Adler.

The chapter devoted to the psychology of rumour and to a consideration of the relation it bears to the psychological forces at work in a crowd, or in an individual, contains some wise criticism of the old classical 'crowd psychology,' and helps to emphasize afresh the very real line of demarcation that there is between the ordinary emotional type of thought, 'complex' thinking, and that rare phenomenon, genuine rational thinking. The discussion should serve as an efficient astringent to all those persons who, like the reviewer, find it difficult consistently to put into practice their knowledge that, in any matter of really vital importance, man is indeed far from being a 'rational animal.'

In the present edition no material alteration has been made in the body of the book. It does however contain a new and valuable chapter on 'The Conception of Dissociation' which permits of a more detailed consideration of this topic than was possible in the original lectures. This, as Dr Hart hoped it would do, renders still more intelligible the point of view he has adopted with regard to the respective contributions to psychopathology of Janet and of Freud.

The Psychology of Insanity. By BERNARD HART. 4th edition. Cambridge University Press. 1930. Pp. v+176. 3*s*. net.

The fourth edition of this valuable and well-known book hardly needs a further introduction to psychologists. As Dr Hart explains in the preface to this edition, he has made no material alterations in the original text of 1912. He has refrained from doing this, first because the book is mainly concerned with elementary and general principles which have not been materially affected by advances in subsequent years, and secondly because the development of the subject has been so luxuriant and so complicated that to bring it properly up to date would require a new book.

In order, however, to acquaint his readers in a cursory form with the significance of the principles he originally described, to the structure and development of modern psychology and psychopathology, he has written an introduction or postscript of nineteen pages, in addition to incorporating in the body of the book a series of explanatory footnotes, together with a brief bibliography of some of the more recent psychopathological works. In the introduction he is mainly concerned with critically assessing the importance, from a psychopathological point of view, of the work of Janet, Freud, Jung and Adler.

An Introduction to Abnormal Psychology. By V. E. FISHER. New York: The Macmillan Co. 1929. Pp. x + 512. 11*s*. net.

Prof. Fisher begins his study of the abnormal by considering in some detail how very varied and how maladaptive the responses of the normal individual often are when he is faced with a difficult situation. From this it is but a step to the abnormal responses of those who are regarded as suffering from mental disorders. The various psychoneuroses and psychoses and the many theories which have been put forward to explain them are then considered. In each case it is shown that the illnesses are in reality disorders of personality, and can best be understood as such. The book ends with chapters on sleep, dreams, suggestion and the feeble-minded. There is no mention of therapy.

The volume is a useful, well-balanced introduction to the psychology of the abnormal.

Individual Diagnosis. By F. G. CROOKSHANK. Psyche Miniatures, Medical Series, No. 13. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. 89. 2*s*. 6*d*. net.

This little book forms one of a series which considers the connection between psychology and medicine, and it is of interest primarily to medical students.

On the assumption that the art of medicine consists of diagnosis, the question is raised in the first lecture, entitled 'Diagnosis and the Syndrome,' as to what diagnosis is. Dr Crookshank points out that once it has been grasped that diagnosis in terms of disease is but the adoption of one diagnostic convention—and that not necessarily the best—a great stride has been taken in the making of a physician! For the consequence of accepting Galen's alternative definition, namely that diagnosis is the thorough comprehension or understanding of all things present, is of immense importance to the outlook and methods of a medical practitioner, since it presupposes the realization of what Crookshank insists is to a doctor still a revolutionary fact, that patients rather than diseases should be the objects of his study. There follows a good deal of discussion about the nature and value of the conception of a symptom-complex or syndrome, and the essay concludes with a plea for the importance of the empirical method in medicine, and an outline of an 'individualized' diagnosis convention involving diagnosis in terms of the syndrome, together with a statement of all that can be found wrong with the patient.

The second lecture is entitled 'Organ-Jargon; or Organ States and Emotional Correlatives.' It supplies a series of interesting historical and contemporary diagnostic studies, and interprets these from the point of view of 'Human Medicine' in accordance with the diagnostic conception outlined in the preceding lecture.

Civilization and its discontents. By SIGMUND FREUD. Translated by JOAN RIVIERE. The International Psycho-Analytical Library, No. 17. London: Hogarth Press, and the Institute of Psycho-Analysis. 1930. Pp. 144. 8*s*. 6*d*. net.

Perhaps the chief value of this book lies rather in its literary artistry than in any very new contribution which it makes to the science of psychology as such. Undoubtedly it is very good reading; there is at times an almost emotional beauty in the dignified lucidity of its balanced prose, and it presents in a brief form an admirable general impression of the Freudian view-point.

After a preliminary consideration of the source of the religious spirit in man, which Freud believes to be based upon the child's initial need for a father's protection, it passes on to a discussion of the complicated forms in which this need persists in adult life, principal among these being the negative necessity to avert as best one can the pain and frustration inflicted upon the ego by the outer world. Intoxication, mysticism, neurotic illness, the sublimations of the artist or the scientist, phantasy-pleasures like the enjoyment of art and beauty, the personal delusions of the hermit or the fanatic, together with the mass delusions of organized religions, all these paths by means of which man endeavours to attain an incomplete happiness are discussed and evaluated, the significance of work and of love in this connection being stressed in a particularly vivid if not new way.

The realization of the inadequacy of our present civilization or culture to regulate, without inflicting needless individual suffering, the human relationships it alone makes possible, leads to a consideration of the nature, origins and evolution of culture or civilization. In the course of this there appear four interesting psycho-analytical surmises. The first of these is concerned with the origin of man's acquisition of power over fire; the second serves to explain woman's, as distinct from man's, inimical attitude to the claims of culture as opposed to the interests of sexual life. The third and fourth surmises are put forward in support of Freud's controversial suggestion (for which no evidence is forthcoming), that there has been a perceptible decrease in the importance of sexuality as a source of pleasurable sensations—*i.e.* as a means of fulfilling the purpose of life; this apart altogether from the unhappy effects of what he considers the grievously unjust and narrow attitude towards sexual love which, theoretically at least, has for so long prevailed in the civilization of Western Europe.

To Freud the evolution of culture comes finally to mean the struggle between Eros and the death instinct, or the destructive aspects of the tendency of individual aggression. Culture, erotically impelled to bind mankind into closely knit masses, renders the strong individual craving for aggression innocuous by introjecting or externalizing it. In this form as a super-ego or conscience it finds an outlet by exercising against itself the same harshness it would have liked to exercise against others. And out of this tension between the strict super-ego and the subordinate ego, arise those extraneous phenomena so essential to any rigid preservation of social solidarity, the sense of guilt and the need of punishment. There follows an elucidation, along familiar Freudian lines, of the complicated dual origin of this sense of guilt, of its immense cultural significance, and of its dangers.

The book concludes with a critical examination of the relationship to one another of the two analogous processes, the development of the individual and the cultural process evolving in humanity. Freud does not regard the struggle between the individual and society as necessarily irreconcilable, *i.e.* not as derived directly from the antagonism of the primal instincts of Eros and Death. At the present time, as a result of maladjustments, and through partial ignorance of the real significance of certain human needs, the defences against individual aggression seem to cause almost as much misery as the aggression itself, but however greatly restrictions and renunciations may oppress the lives of individuals to-day, he believes that they do admit of solution in the future of civilization.

Sisyphus: or the limits of Psychology. By M. Jaeger. To-day and To-morrow Series. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1929. Pp. 94. 2*s.* 6*d.* net.

The author tilts a lance with Freud and with Watson, but his attack is not conceived with much originality, and his victories are pyrrhic. Dostoievsky, Hamlet, Pirandello and others lend humanity to the theoretical discussion, and there is an interesting paragraph on the eleven-hour day in a Detroit motor-works. Salesmen and advertisers also play their part in the argument, but the ultimate appeal is to the metaphysician: can psychologist Münchausen lift himself out of the mud by his own pig-tail? This book will not help much—it should amuse.

Les Causes du Suicide. Par MAURICE HALBWACHS. Paris: Félix Alcan. 1930. Pp. viii + 520. 70 fr.

Prof. Halbwachs, known already for his most interesting study of the social framework of memory, has here produced another important contribution to social psychology. He deals with the most common forms of suicide in the modern community in relation to age, sex and occupation; gives details of the distribution of suicides in Europe and particularly in France, Germany, Italy and England; considers the relative frequencies of suicide in town and country; discusses the influence of family and religious life; compares suicide with murder; demonstrates the effect of war and of social and economic crises, and considers rather fully the predisposing conditions of a pathological order. He is very strongly of the opinion that suicide is primarily a social phenomenon and that by far the most important feature in its causation is social maladaptation. "We go," he says, "farther than Durkheim along the path which he essayed to travel. For we explain by social conditions not only the great factors which lead to suicide, but even all those special circumstances of the particular case which are not mere pretexts but genuine motives." The work is well documented, clear and interesting. It merits the very serious attention of all social psychologists.

Psychology and Religion. A Series of Broadcast Talks. By E. S. WATERHOUSE. London: Elkin Mathews & Marrot. 1930. Pp. xix + 232. 5*s.* net.

In the nature of the case this cannot be a very original contribution to the psychology of religion. But it is nevertheless a very good book indeed, having regard for the purpose behind its preparation. Dr Waterhouse has an adequate grasp both of the achievements and of the limitations of psychology within the field with which he is dealing. He has also a straightforward and attractive style of exposition. That his own interests run well beyond the psychological explanation of religious behaviour and experience to a strong belief in religious values he never seeks to disguise. But for all except the most hopelessly biased this increases the attractiveness of this very honest and readable book. Eighteen 'talks' dealing with most of the generally discussed problems of religion that are amenable to psychological treatment are included.

The Art of Study. By T. H. PEAR. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. vi + 114. 3*s.* 6*d.* net.

Although this little book is meant for the general reader rather than for the student of psychology, the latter will gain not only much pleasure but a large number of attractive hints for his further study if he gives it some serious attention.

Prof. Pear is largely concerned with problems of learning and recall. He makes it perfectly clear that he is well acquainted with the mass of experimental work that has been done on these and related topics, and admirably selects from the available material that which is most important and relevant to his purpose. The book abounds with practical hints, and it is written with rare clarity and vivacity.

L'Activité Psychique chez les Animaux. Par L. BRETEGNIER. Paris: Vigot Frères. 1930. Pp. xii + 386. 50 fr.

The book deals chiefly with problems of instinct and intelligence in animals. After an historical study of early views concerning mind in animals, the author gives an account of modern behaviouristic methods of experiment. He then proceeds to deal with tropisms, reflexes and their interconnections, and instinct. Some of the most interesting parts of the discussion of instinct concern modes of orientation of animals in space. A special chapter is devoted to a study of social insects. A rather brief study of emotional and affective reactions in animals is followed by the main chapter of the whole book. This discusses animal intelligence. Nothing very new either in the way of fact or theory comes out of this, but a great amount of information of modern investigation and observation has been brought together. A final short chapter deals with language in animals. In general the author finds reason to maintain that animal intelligence though qualitatively much like that of man is quantitatively strikingly inferior. And in a familiar way he sets this down in the main to man's superior use of language. The book is not an original study, and it could be greatly condensed with advantage. Yet it certainly collects a vast mass of material from which a discerning reader will be able to gather much that is suggestive and enlightening.

Readings in Psychology. By R. H. WHEELER. New York: Thomas Y. Crowell Company. 1930. Pp. x + 597. $3.75.

There can be no doubt that this will prove a very valuable book in the hands of any discerning student who requires to get a fair knowledge of contemporary experimental work in psychology, or who is on the look out for research problems. It consists of a large number of selected 'readings' extracted from first-hand contributions to the various topics dealt with. These readings fall under seven groups: Social Behaviour; Intelligent Behaviour; Emotive Behaviour; Learning; Reaction and Observational Behaviour; Observational Behaviour; and the Nervous System. On the whole the readings are extremely well selected and they are all supplemented by further references. Though the plan of the book follows the editor's *Science of Psychology,* the readings may easily be made to serve the purposes of any student or teacher who may be using a different text. Both in plan and in performance Prof. Wheeler has done good service to psychology in this volume.

The Enlargement of Personality. By J. H. DENISON. London: Charles Scribner's Sons. 1930. Pp. xvii + 340. 10*s.* 6*d.* net.

"Here," say the publishers, "is explained a successfully tested method for becoming the person you would like to be, for obtaining happiness, peace, or whatever your *real* objective has been." The *recipe* is simple. We are what we think we are. What we think we are is in fact a heritage from a long-gone social history. Change our views of ourselves and we change ourselves. The book is popular and makes a great parade of anthropological knowledge. It cannot very well all be wrong: it certainly is not all right. The most striking thing about it is the very clever dust sheet provided by the publishers.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

October 25, 1930.	"Unseen Drama and Imagery: Experimental Observations." A paper by Miss Madeline Kerr, B.A. "Psychological Problems suggested by Radio-Drama." A paper read by Professor T. H. Pear.

Medical Section.

May 28, 1930.	"The Historical Background of the Problem of Temperament." A paper read by Miss May Smith.
June 25, 1930.	"Organ-Jargon." By Dr F. G. Crookshank.
October 22, 1930.	"Emotion from the Neurological Standpoint." A paper by T. R. Hill, M.D. (Lond.), M.R.C.P. (Lond.).

Education Section.

June 2, 1930.	"Some Observations on the Speech of a Young Child." A paper read by Mrs E. Norman, M.A.
October 20, 1930.	"An Investigation on the Use of Intelligence Tests with University Students." A paper contributed by Miss Barbara Dale, M.A., Ph.D.
November 10, 1930.	A Symposium on "The Nature of Stammering." Opened by Millais Culpin, M.D., F.R.C.S. and E. J. Boome, M.B., Ch.B., D.P.H.

Industrial Section.

May 29, 1930.	"Sickness Records." A paper read by Millais Culpin, M.D., F.R.C.S.
June 19, 1930.	"Personal Qualities in Accident Causation." A paper read by E. G. Chambers, M.A.
October 27, 1930.	"Perseveration as a means of testing Leadership." A paper read by W. J. Pinard, M.A., Ph.D.

Aesthetics Section.

July 18, 1930.	"The Nature of Prose." A paper read by Mrs Ursula Roberts (Susan Miles).

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PHENOMENAL REGRESSION TO THE REAL OBJECT. I.

BY ROBERT H. THOULESS.

(*From the Department of Psychology, Glasgow University.*)

I. *The apparent shapes of figures observed obliquely* (pp. 339–347).
II. *Hering's observations on 'Gedächtniss-farben'* (pp. 347–351).
III. *Experiments on apparent sizes* (pp. 351–355).
IV. *The tendency to equalize vertical semi-axes in perception of the inclined square and the phenomenal regression of parallel lines* (pp. 355–358).
V. *Summary* (pp. 358–359).

I. THE APPARENT SHAPES OF FIGURES OBSERVED OBLIQUELY.

IT is commonly stated in textbooks of psychology that when we observe figures inclined to us, we see them not in the shapes indicated by the laws of perspective but in the shapes which these figures 'really' possess. Thus when we look obliquely at a circular object, we see it not as an ellipse but as a true circle[1]. While it is undoubtedly true that such an object seen in these conditions is judged to be of its true shape and also that we are prepared for motor reaction to a circular object, I do not find that experiment confirms this statement as to what shape is seen. If a subject is shown an inclined circle and is asked to select from a number of figures the one which represents the shape seen by him, he chooses without hesitation an ellipse. This ellipse, however, is widely different from the one which represents the shape of the inclined circle indicated by the laws of perspective, being much nearer to the circular form. The subject sees an inclined figure neither in its 'real' shape nor in the shape which is its perspective projection but as a compromise between these.

This result is equally at a variance with the view widely held by those who are not psychologists that the perceived characters of an object are those of its projection on the retina. This view is particularly to be found amongst writers on perspective. Thus the writer on perspective in the 14th edition of the *Encyclopaedia Britannica* (2) sums up the laws of perspective in a series of 'axioms' which are not given as axioms about the plane projection of solid figures but of how we perceive them. Thus: "Axiom 1. Parallel lines appear to approach one another as they vanish, and to meet at an infinite distance from the observer in an imaginary point called the vanishing point of the system. Axiom 2. Parallel planes appear to approach one another as they recede from the eye, ..."

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[1] H. J. Watt, for example, says: "Generally a plate looks what it 'really' is, circular, not elliptical as the retinal image of it really is (1)."

What this writer regards as axiomatic is that the characters of perception are identical with those of peripheral stimulation. While receding parallel lines do appear to converge, the proposition that they converge in appearance in the same way as they do in the projection on the retina or in a photograph (*i.e.* to the perspective vanishing point) is not only not axiomatic but experiment shows that it is not true.

The first experiments on apparent shapes were done with a subject looking from a controlled height at a circular or square disc lying on a table at a measured distance from the vertical line through the subject's eyes (Fig. 1). The purpose was to discover the apparent shape of the object from this point of view and to compare this with the shape of the perspective projection of the object on to a plane at right angles to the line of sight (*i.e.* with the shape which it would have in a photograph or in a drawing made in accordance with the laws of perspective). For the sake of brevity, I shall refer to this as the 'perspective shape' or 'stimulus shape.' The shape reported by the subject as seen by him may be called the 'apparent shape' or 'phenomenal shape.' This was at first measured by asking him to draw the disc as it appeared to him from that point of view. Later, it was found better to make the subject match the apparent shape of the circle with one of a series of ellipses cut out of cardboard with different ratios of short to long axis[1]. This ratio differed by 0·05 in successive ellipses. These ellipses were presented successively

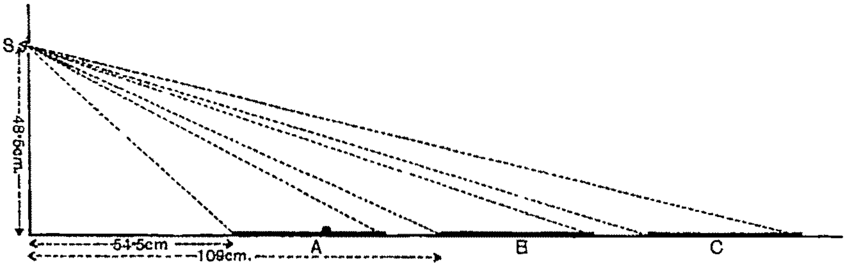

Fig. 1.

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[1] The perspective shape is not, of course, exactly an ellipse, but a figure resembling an ellipse with one of the short semi-axes longer than the other. The difference, however, is small and, since the judgments of the subject were only with respect to the relative lengths of the axes, it is of no importance for the purpose of the experiment.

to the subject (using the method of complete ascent and descent) and he was asked to judge whether the presented ellipse was 'fatter,' 'thinner' or 'the same as' the apparent shape of the circular disc. Preliminary practice was given, and the usual precautions of psychophysical experimentation were taken. These two methods I shall refer to as the 'drawing method' and the 'matching method,' respectively. Since it was no part of the aim of the drawing method to test the subject's drawing ability, he was allowed to alter his drawings as he pleased or to start them again until he had produced one which he was satisfied represented the shape as he saw it. In all experiments, except when otherwise stated, observation was with both eyes fully open and focussed on the object. The real shape of the disc used was therefore known by the subject.

The objects used were a white cardboard circle of 39·75 cm. diameter and a square of diagonal 38·0 cm. The object used lay on a dark table and was observed by the subject with his eyes 48·5 cm. above the end of the table (Fig. 1). The square was always placed with one of its diagonals in line with the subject. Three positions were experimented with: *A*, in which the nearest point of the object was 54·5 cm. from the point below the subject's eyes; *B*, in which it was 109 cm.; and *C*, in which it was 163·5 cm. from the same point.

Tables I and II show respectively the results for the subject S. drawing the circle and the square respectively. Table I shows all results (except of preliminary practice); Table II shows mean results of nine experiments at each position of the object. The figures given in the first and second columns are the ratios of short to long axis in the reproduced

Table I. *Drawings of circle by subject S.*

	Circle at *A*			Circle at *B*			Circle at *C*		
	Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression
(1)	·755	·56	·50	·575	·36	·46	·455	·255	·42
(2)	·76	·56	·53	·60	·36	·50	·445	·255	·41
(3)	·82	·56	·66	·56	·36	·43	·51	·255	·49
Mean	·78	·56	·57	·58	·36	·465	·47	·255	·445

Table II. *Mean of nine drawings of square by subject S.*

Square at *A*			Square at *B*			Square at *C*		
Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression	Reproduced ratio	Perspective ratio	Index of phenomenal regression
·86±·03	·565	·74	·73±·04	·36	·69	·58±·035	·255	·60

figure and in the perspective shape respectively; those in the third columns are measures of the degree to which the 'real' determines the 'seen' shape, calculated by a formula explained below.

The same results are shown diagrammatically in Fig. 2. In each diagram the inner blackened figure shows the perspective shape of the object, the outer broken line marks its true physical shape, while the continuous line shows the mean reproduced figure (*i.e.* its phenomenal shape). In all cases it will be seen that the reproduced figure lies between these two extremes, and that it sometimes lies nearer to the physical shape than to the perspective shape. It is as if the known physical shape of the object distorted towards itself the seen shape from that which we should expect to result from the sensory cue of the image on the retina.

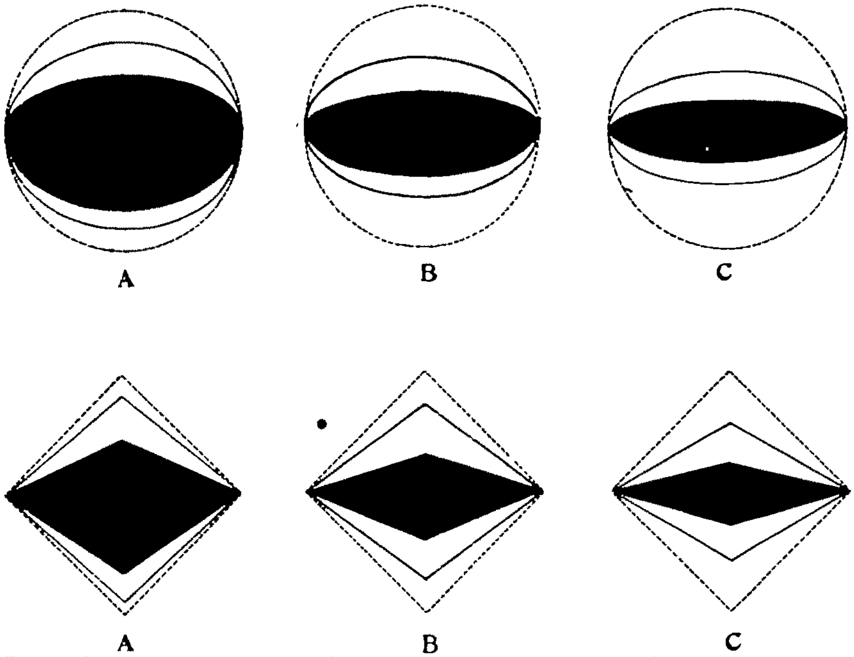

Fig. 2. S.'s mean reproductions of circle and square. Broken line shows physical shape of object. Black figure shows its perspective shape. Continuous line shows mean shape of reproduction.

The first doubt that occurs to one's mind in attempting to explain these results is whether it is not possible that in all reproductions of ellipses or of trapezia there is a tendency to revert to the circular or square form. If this were the case, the assimilation to these shapes here found would not be the result of the stimulus being a projection of a physically circular or square object but would be characteristic of any reproduction of an ellipse or a trapezium. This, however, was proved not to be the case by a subsidiary experiment.

The subject was given actually elliptical discs to reproduce from normal, (*i.e.* not inclined) observation. The three ellipses given to the subject to copy had ratios of short to long semi-axis of 0·7, 0·45, and 0·25. The means of three reproductions by drawing of these were respectively 0·69, 0·45, and 0·245. Thus there is no tendency to revert to the circle in copying an ellipse[1]. The tendency observed when drawing an inclined circle or square must, therefore, be due to the effect of the actual shape of the physical object.

This experiment also eliminates the possibility that any appreciable part of the observed effect is due to irradiation of the white surface over the darker background. Any such action would take place equally whether the stimulating object were an ellipse viewed normally or an inclined circle giving the same retinal image.

In this failure of seen shapes to obey the laws of perspective, we are reminded of analogous phenomena in perception. Hering (3) showed that a white disc in shadow may appear brighter than a strongly illuminated grey disc even though the degree of shadowing is so great that the white is actually reflecting less light to the eye than is the grey disc. Apparent brightness is thus determined partly by the 'real' brightness or reflectivity of the object seen and not solely by the intensity of the retinal image. Similarly, if two objects of the same shape but different size are placed at such distances from the eyes that their apparent sizes are equal, it is found that their relative distances are such that the retinal image of the 'really' larger object is considerably smaller than that of the other. Apparent size is a function of 'real' size as well as of size of retinal image. In later sections, it will be shown that in these two cases also the same law of compromise holds. Under ordinary conditions of binocular vision, the actually experienced character of the object (or the 'phenomenal character') is a compromise between the 'real' character of the object and the character given by peripheral stimulation, whether this character is shape, relative size, or relative brightness. In all of these cases, the phenomenal character shows a tendency away from the stimulus character towards the 'real' character of the object. As a general name for this tendency, in whatever kind of perceptual character it is found, we may use the term *phenomenal regression to the 'real' object* or, more shortly, *phenomenal regression*.

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[1] There is, however, distortion in copying ellipses with larger ratio of short to long semi-axis. An ellipse with short axis vertical and of ratio 0·95 was copied as an ellipse of ratio 0·93, while a true circle normally observed was copied as an ellipse of ratio 0·975 (short axis vertical). The distortion is small and in the opposite direction to that due to phenomenal regression. It is probably due to the 'horizontal-vertical illusion.' It was one reason for later abandoning the drawing method in these experiments. The other reason was the possibility that training in drawing may tend to condition the drawing response to the stimulus shape even though the subject's perception of the object would be intermediate between the stimulus shape and the 'real' shape if this perception were tested by some form of response in which the subject had received no previous training.

It is also convenient to have a numerical measure, applicable to any perceptual character, of the degree to which this regression takes place. Let us use the symbol S for a stimulus character (*e.g.* the ratio of short to long axis in the perspective shape of square or circle in the above experiments), the symbol P for the corresponding phenomenal character (the corresponding ratio in the figure matched or drawn by the subject), and R for the corresponding 'real' or physical character of the object (the ratio in the actual object—unity in the square or circle). An obvious measure of the degree of regression of the phenomenal character away from the stimulus character towards the 'real' character of the object is the fraction of the distance separating the real from the perspective character over which the phenomenal character has regressed: *i.e.* the fraction $(P-S)/(R-S)$. This proves, however, not to be a satisfactory measure, since it leads to certain anomalies (particularly when used for the brightness and size regressions). A formula, only a little more complicated, which is free from these difficulties is

$$(\log P - \log S)/(\log R - \log S).$$

This is the measure which I have used throughout these experiments and have called the *index of phenomenal regression*. Its value is zero if there is no phenomenal regression, that is, if the phenomenal character is identical with the stimulus character; while it is unity if regression is complete, that is, if the phenomenal character coincides with the 'real' character of the object.

Determinations of the phenomenal shape of the inclined circle were also made with other subjects by the matching method. Mean results by this method with S. and other subjects are shown in Table III. It will be noticed that there are considerable individual differences in the amount of phenomenal regression.

The results (Table III) show that the index of phenomenal regression varies for different inclinations of the object to the line of vision. It seems also to vary somewhat with the size of the object, its distance and its shape (whether square or circular). The three latter sources of variation were not investigated. An experiment was performed, however, to discover how the amount of phenomenal regression varied with different angles of inclination of the object. For this purpose, a white circular disc of 29·7 cm. diameter was mounted on a turntable with its axis of rotation horizontal and at right angles to the subject's line of vision. The disc was mounted with a diameter in line with the axis of rotation of the apparatus, so that turning the apparatus presented the disc to the subject at varying angles of inclination. The centre of the disc was 142·5 cm. from the subject's eyes. Eight observations in each position were made with the subject S. by the matching method at approximately the following angles of inclination: 7°, 10°, 20°, 30°, 45°, 65°, and 90°. The angles could not themselves be measured with sufficient accuracy for exact calculation of their perspective shape in each position, so this was done by placing a camera in the position of the eyes and measuring the ratios of the photographed ellipses with a travelling microscope. The results of this experiment are shown as a graph in Fig. 3.

Table III. *Mean matchings of phenomenal shape of inclined circle by four subjects.*

	Circle at *A*			Circle at *B*			Circle at *C*		
Subject	Matched ratio	Perspective ratio	Index of phenomenal regression	Matched ratio	Perspective ratio	Index of phenomenal regression	Matched ratio	Perspective ratio	Index of phenomenal regression
S.	> ·7*	·56	> ·39	·495	·36	·31	·46	·255	·43
X.	·74	·56	·48	·59	·36	·50	·43	·255	·38
B.	·785	·56	·58	·56	·36	·43	·435	·255	·39
D.	·84	·56	·70	·725	·36	·685	·645	·255	·68
M.	—	—	—	·575	·36	·46	—	—	—

* No standard ellipse of larger axis-ratio than this was available at the time of experimenting with S.

In this figure, the broken line shows the variation of phenomenal shape with perspective shape. If the seen shape were identical with the perspective shape, all observed values would fall on a straight line passing through the origin and inclined at an angle of 45° to each axis. A continuous line has been drawn in this position. The amount of phenomenal regression is, therefore, shown by the height of the broken line above the continuous line. The diagram indicates that the amount of phenomenal regression diminishes to zero as the angle of inclination approaches 90° and (less certainly) as it approaches 0°. The indices of phenomenal regression are as follows: 7°, 0·28; 10°, 0·41; 20°, 0·33; 30°, 0·32; 45°, 0·16; and 65°, 0·1.

It has already been shown by the earlier experiment in reproduction of actually elliptical figures viewed normally that these results are not the consequence of a tendency to prefer the circular shape in perception and to assimilate other closed curves to the circular form in reproduction. This is shown more strikingly in the following experiment, in which the regression was from the circular shape and not towards it.

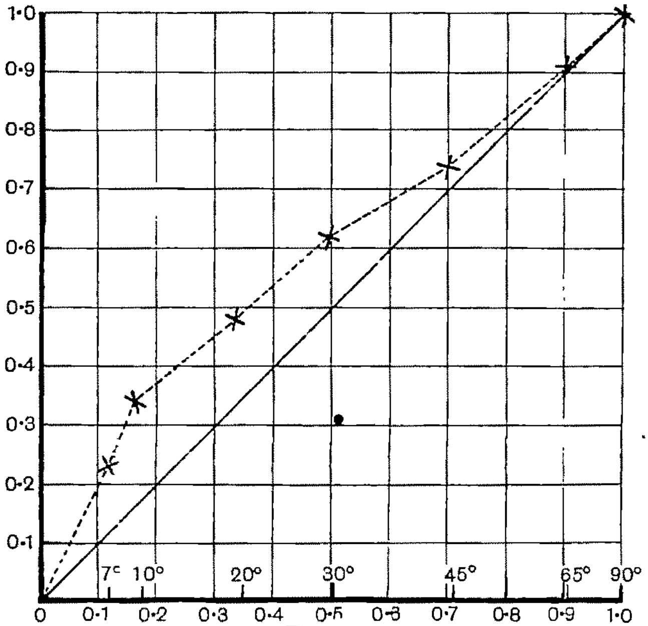

Fig. 3.

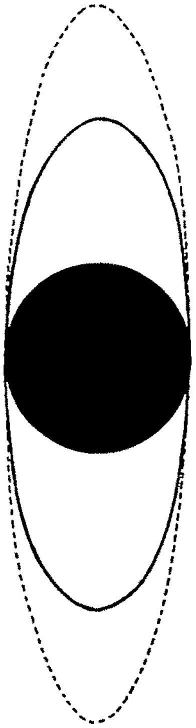

Fig. 4.

Fig. 3. Variation of phenomenal shape and of index of phenomenal regression for different inclinations of circular disc to line of vision. *Horizontal axis.* Ratio of axes in stimulus shape (corresponding angles of inclination shown above the axis). *Vertical axis.* Ratio of axes in phenomenal shape.

Fig. 4. S.'s mean matching of phenomenal shape of elongated ellipse so arranged that its perspective shape is approximately circular. Broken line shows physical shape of object. Black figure shows its perspective shape. Continuous line shows mean phenomenal shape.

Instead of using a circle as stimulus object an ellipse was used with long axis pointing away from the subject and the ratio of the axes was so chosen that the perspective figure would have equal axes (*i.e.* would be as nearly a circle as it is possible to get with a perspective projection of an ellipse). The actual shape of ellipse necessary in the position used was found to have a ratio of long to short axis of 3·95. The result of this experiment is shown in Fig. 4. If the tendency were simply to assimilate the phenomenal figure to a circle, then no distortion should take place. In fact, there is still distortion and its direction is away from the circle and towards the physical shape of the object perceived—the ratio of the phenomenal shape was 2·65. It is the physical shape of the object, therefore, and not a preference for the circular form that determines the change in the phenomenal shape.

It is also possible, of course, to arrange an elliptical object so that the phenomenal shape is itself circular. The condition for this is that a sufficiently elongated ellipse with its long axis pointing from the subject shall cast on the retina an image of an ellipse with long axis horizontal. This case is of no special theoretical interest[1].

The full series of experiments from which the example in Fig. 4 was drawn is shown in Table IV. The ratios in columns 2 and 3 are of vertical to horizontal axes, in column 1 of axis in line with subject to that at right angles to him. It will be seen that not only is there no tendency for phenomenal regression to diminish as the perspective shape approaches circularity, but even that under those conditions the index found was greater than with any other perspective shape.

Table IV. *Mean matchings of phenomenal shapes of various elliptical discs viewed obliquely.* (*Subject S.*)

Ratio in observed disc of axis in line with subject to that at right angles to him	Ratio of vertical to horizontal axis of perspective shape	Mean ratio of vertical to horizontal axis of phenomenal shape	Index of phenomenal regression
3·93	1·05	2·65	0·70
2·01	0·54	1·19	0·60
1·33	0·355	0·825	0·64
1	0·27	0·50	0·47
0·75	0·20	0·40	0·52

II. HERING'S OBSERVATIONS ON *Gedächtniss-farben.*

In his *Grundzüge der Lehre vom Lichtsinn* (3), Hering records a series of observations on the perception of relative brightnesses and colours, of which the following is typical. If a piece of grey paper is placed near a window and compared with a piece of white paper so much farther away from the window that its luminosity is less than that of the grey paper, the grey paper is, nevertheless, seen as grey and the white as white—*i.e.* the less luminous surface of the white paper is seen as the brighter. If the two pieces of paper are now simultaneously examined with one eye looking through a blackened tube, the apparent brightness relationship is reversed, the white paper appearing to be of a darker shade of grey than the other. If they are again examined as part of the full field of vision of both eyes, the brightness relationship is seen as at the beginning—the less luminous white paper is seen as the brighter.

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[1] It is, however, one of the most convenient methods of demonstrating the effect. If we discover the ellipse which, with its long axis pointing from the subject, appears to him at a given inclination approximately circular (*i.e.* with equal horizontal and vertical axes), we can demonstrate to the subject the fact that the perspective figure is really a flattened ellipse by allowing him to look at the object through a circular aperture held at right angles to the line of vision at such a distance from the eyes that the longest axis of the perspective ellipse just fits inside the circle.

Similar effects were observed with tinted objects whose apparent hues were found to tend to remain constant in spite of changes in colour of the illuminating light. These shades or hues, which persisted in spite of changes in illumination, Hering called *Gedächtniss-farben* (usually translated as *memory colours*), and he picturesquely describes us as seeing certain classes of objects "through the spectacles of memory colours."

A mere repetition of the experiment on brightness described above might lead us to a conclusion which a more careful study shows to be wrong. The white paper appears brighter than the grey, although so much less illuminated that its luminosity is actually less. We might be led to suppose, therefore, that there is a law of absolute 'constancy of brightness' by which the paper of greater reflectivity[1] appears the brighter under any illumination however much reduced.

Further experiment, however, shows that the matter is less simple. If the white paper is further shadowed, a point is reached at which it appears of the same brightness as the grey paper, and if the reduction of its illumination is carried beyond this point, the white paper appears the darker. We are again dealing with a compromise effect, the apparent brightness is neither determined solely by the 'real' character of the paper's reflectivity nor solely by the stimulus character of luminosity, but is a compromise between them.

The fact of this compromise may be made clearer by a quantitative example. I took two squares of paper (20 cm. × 20 cm.) vertically mounted on cardboard. One was white, *A* (slightly brighter than the no. 1 of Zimmermann's scale); the other was a light grey, *B* (between

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[1] I am using this word 'reflectivity' for the position of the paper itself on the white-black scale in order to avoid the ambiguity attached to the term 'physical brightness.' Psychologists commonly refer to this quality as the 'brightness' of the paper. Physicists, however, generally use 'brightness' in the same sense as 'luminosity' for the intensity of reflected light under given conditions of illumination. For this I shall use the word 'luminosity,' reserving 'brightness' for the phenomenal character.

Zimmermann's 3 and 4). Their relative reflectivities were first determined by rotating a sector of the white before a blackened chamber and matching with the grey. The amount of the white required for a match was found to be 135·5°, so the reflectivity of *A* was 2·65 times that of *B*. The two papers were normally illuminated in a darkened room by a 4-volt (1·2 watt) electric bulb in a blackened case with a filament of size negligible compared with the distance of the papers. The intensity of illumination of the papers was, therefore, inversely proportional to the square of their distances from the lamp.

The subject S. looked at the two papers with both eyes open and his head in such a position that the papers were not immediately adjacent but separated by a short space of black background. *B* was 100 cm. from the lamp and the mean distance at which *A* was found to be of equal phenomenal brightness was 184 cm. At a greater distance, *A* appeared the darker. At this distance of 184 cm., the calculated luminosity of *A* was 0·785 that of *B*. In other words, a paper about 2½ times as reflective as another appeared equally bright to binocular observation when its actual luminosity was about three-quarters that of the other.

The white paper was next adjusted to phenomenal equality with *B*, but observed under different conditions. The subject looked with one eye through a blackened tube which cut out all surrounding objects, and with his head in such a position that he saw through the tube the surfaces of the two papers adjacent to one another. The mean distance of *A* at which the papers appeared equally bright was now 162·5 cm. Calculation shows that the ratio of the luminosity of *A* to *B* was now 1·0035 : 1; that is, the luminosity of the two papers was, as nearly as possible, equal.

A similar experiment was performed with the same subject on another day with a paper *C* of a much darker grey (no. 41 of Zimmermann's brightness scale of 50 shades). This was adjusted by the same light to phenomenal equality with each of the other two papers at 507·5 cm. from the same lamp as was used in the earlier experiment. In this case, the reflectivity of *C* was not determined independently but was assumed to be given by adjusting to apparently equal brightness with monocular observation through a blackened tube. In this experiment the effect of the inclination of *C* to the source of light was not negligible, so its luminosity was calculated from the formula:

$$\text{luminosity} \propto \text{reflectivity} \times \frac{\cos\theta}{d^2}.$$

The results of this experiment and of the earlier one are given in Table V.

Table V. *Phenomenal regression of papers of differing reflectivity so illuminated that they appear of equal brightness.*

Papers used	R_1/R_2	P_1/P_2	S_1/S_2	Index of regression
A and *B*	2·65	1	·785	·19
B and *C*	4·95	1	·298	·43
A and *C*	13·1	1	·175	·405

We are here dealing with an effect analogous to that of the phenomenal regression of perceived shapes. The relative reflectivity of the papers is the physical property of the papers themselves corresponding to the 'real' shapes of perceived figures. Their relative luminosity (which is equal to the relative luminosity of the retinal images) is the character of peripheral stimulation corresponding to the perspective shapes of the figures. Their relative phenomenal brightness (adjusted in this experiment to unity) corresponds to the phenomenal figure. The effect observed is that the relative phenomenal brightness is not determined solely by the relative stimulus brightness (*i.e.* relative luminosity) of the papers but also by their relative 'real' brightness (*i.e.* relative reflectivity). The relative phenomenal brightness is, in fact, a compromise between these two. As was observed with the perception of shapes, the greater the difference is between the reflectivities of the papers used, the greater must be the difference in their relative luminosities if they are to appear equally bright. If we indicate the relative reflectivities, luminosities, and phenomenal brightnesses by the symbols R, S and P respectively, the index of regression is given by the formula

$$(\log P - \log S)/(\log R - \log S),$$

or (since P is unity) by $(-\log S)/(\log R - \log S)$.

In a beautiful and ingenious series of experiments Köhler (4) has shown that both chimpanzees and hens show the tendency to constancy of brightness. He demonstrated that when these animals are trained to react to the brighter of two papers equally illuminated, they continue to react to the more reflective paper even when its illumination is so much reduced that it has a lower luminosity. He did not, unfortunately, investigate the further problem of the maximum degree of lowered luminosity of the whiter paper at which it still called out the same response and beyond which it was reacted to as the darker paper. It is not, therefore, possible from his results to calculate an exact value for the phenomenal regression but only a value which it must have exceeded.

With chimpanzees, the greatest disparity of illuminations was when the whiter paper (no. 3 of the Zimmermann scale) had a luminosity

0·0795 that of the darker (no. 41). If we take their relative reflectivities as about 5, this gives an index of regression greater than 0·61. There is some indication that the stimulus difference is here reaching the value at which reversal of the reaction would take place, since the ape makes two wrong reactions out of ten (although he makes none at all when the stimulus difference is less). It is, therefore, probable that the index of regression does not much exceed this value.

With hens there was no sign of an approach to the point of reversal when the relative stimulus values were 0·0807 : 1 with the papers 3 and 30 of Zimmermann's scale. I do not know the relative reflectivities of these papers, but since their difference is less than that between the papers used for the apes, the index of regression for the hens must be considerably greater than 0·61.

Katz (5) has used human subjects in an experiment in which rotating colour wheels with black and white sectors were adjusted to phenomenal equality when one was in shadow, and obtained results which give indices of phenomenal regression ranging in one experiment from 0·33 to 0·69.

Obviously, there are insufficient data for determining whether phenomenal regression is greater for chimpanzees and hens than for human beings, particularly since the experiments were not carried out under comparable conditions. The precise determination of the answer to this question would be an interesting problem for an animal psychologist.

III. EXPERIMENTS ON APPARENT SIZES.

Another example of the same character of perception is to be found in the dependence of the apparent size of a seen object not only on the size of its retinal image but also on the physical size of the object, so that of two things producing equal retinal images the one that is more distant and actually larger also appears the larger. In order to measure this effect, two white circular discs of different sizes were fixed upright on movable stands, which were adjusted to different distances from the subject along two graduated lines on a table so diverging that there was no overlap of the retinal images of the two discs. The arrangements for viewing the discs were similar to those shown in Fig. 1. The larger disc remained fixed, while the distance of the smaller disc was varied until the subject reported that the apparent sizes of the two discs were equal. The 'limiting method' of experimenting was used. The perspective sizes of the two discs were then calculated from their diameters and their distances from the subject's eyes.

When the phenomenal sizes were thus adjusted to equality, in all cases the physically larger disc was at such a distance that its perspective or stimulus size (*i.e.* the size of the image actually cast on the retina) was considerably smaller than that of the other. In other words, phenomenal size also is a compromise between stimulus size and the physical size of the object.

Fig. 5 represents the mean of two sets of experiments with the subject S. The discs were 39·7 and 29·7 cm. in diameter, and appeared equal when they were at 240·5 and 117·5 cm. from the eyes respectively. Thus the larger disc, which was only about four-thirds the diameter of the other and a little less than twice its area, had to be slightly more than twice its distance from the subject for the two discs to appear equal. At this distance the diameter of the retinal image of the larger disc would be about two-thirds and its area less than half that of the smaller. In the diagram, the dotted circle (*R*) has been made proportional to the ratio of the physical size of the large disc to that of the small one, the inner blackened circle (*S*) to the ratio of their stimulus sizes, and the continuous circle (*P*) is proportional to their relative phenomenal sizes (*i.e.* to unity). If there were no phenomenal regression, the circle *P* would coincide with *S* whatever might be the size of *R*.

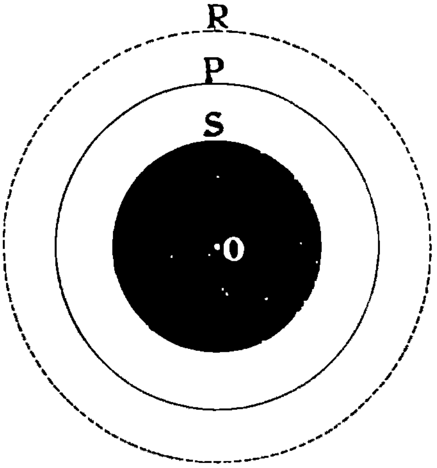

Fig. 5. Conditions for phenomenal equality of two circular discs of different physical size (Subject S.). Broken line shows relative physical size of larger disc. Black figure shows its relative stimulus size. Continuous line shows its relative phenomenal size.

The index of phenomenal regression is calculated exactly as for the experiments in phenomenal brightness. *R* is the relative size of the actual objects, S their relative stimulus size, and P their relative phenomenal size (unity in these experiments)[1].

Table VI shows mean results of this experiment for four subjects. The first two rows were made with the same subject on different days. The subject D. showed so large an amount of phenomenal regression that, when discs of diameters 39·7 and 29·7 cm. were used, the smaller disc was too near the subject for convenient measurement. Discs were therefore used with a smaller difference of size between them. If there were no phenomenal regression, row 8 would be, in all cases, unity. Phenomenal regression is shown by the tendency of the phenomenal diameter ratio (*i.e.* 1) not to coincide with the value in row 8 but to be intermediate between that and the value in row 5.

Table VI. *Mean results of experiments on phenomenal size.*

1 Subject	S.	S.	M.	D.	D.	C.	C.
2 No. of observations from which mean is calculated	12	8	6	—	8	4	8
3 Diameter in cm. of large disc (D_1)	39·7	39·7	39·6	39·7	29·7	39·7	29·7
4 Diameter in cm. of small disc (D_2)	29·7	29·7	29·7	29·7	26·5	29·7	26·5
5 Relative diameters (D_1/D_2)	1·335	1·335	1·333	1·335	1·12	1·335	1·12
6 Distance in cm. of large disc (L_1)	240·5	240·5	230	240	240	240	240
7 Mean distance in cm. of small disc for phenomenal equality (L_2)	120	114	114	< 70	162	98	162
8 Relative perspective diameters (D_1L_2/D_2L_1)	·665	·635	·66	$<$ ·39	·755	·545	·755
9 Index of phenomenal regression $\dfrac{-\log(8)}{\log(5)-\log(8)}$	·585	·61	·59	$>$ ·76	·715	·68	·715

Here also, it is to be noted that we are dealing not with an absolute constancy of phenomenal size but with a tendency to constancy. At no distance from the observer is it true to say that changing distance of the object makes no difference to phenomenal size. As the distance of an object changes, its phenomenal size changes, whether the object be far or near. It changes, however, less rapidly than does the size of the retinal image. The tendency to constancy is shown by the amount of change being a compromise between the changing size of the peripheral stimulus and the unchanging 'real' size of the object.

In order to demonstrate this more fully an experiment was devised in which a light circle was thrown on to a screen 5 m. from the subject, by means of a diaphragm of adjustable size in a projection lantern. A circular white disc supported vertically, of diameter 13·15 cm., was presented to the subject at distances of 1·33, 2, 3, 4, 5, and 6·5 metres. For each of these positions, the subject was required to adjust the circle cast by the lantern until it appeared equal to the disc. Fig. 6 shows the mean of ten observations in each position of the disc.

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[1] It is an advantage of the formula $(\log P - \log S)/(\log R - \log S)$ over the simpler $(P - S)/(R - S)$, that the index has the same value whether the characters used in its calculation are the linear dimensions of the objects, their areas or their volumes. A more important advantage is the fact that it does not matter which of the objects compared provides the numerator and which the denominator for R, P and S. This is not true of the simpler formula. A further objection against the simpler formula is that it leads to absurdly small values of the index if the difference between the real sizes of the objects compared is great.

If the phenomenal size of the disc obeyed the laws of perspective, its changes would be proportional to the changes in stimulus size. The phenomenal length of diameter would, therefore, be proportional to the reciprocal of the distance of the disc from the eyes. All variations of apparent size with distance in Fig. 6 would lead to values lying on one of the series of curves $y = 1/x$, which is the curve of decrease of stimulus size with increasing distance. To show how far this is from being true of phenomenal size, I have drawn a curve $y = 1/x$ through each of the recorded values of the phenomenal size. The curve of change of phenomenal size cuts across these curves and follows a course intermediate between them and the curve of absolute constancy of shape (shown as a thin straight line parallel to the base through each recorded value). The

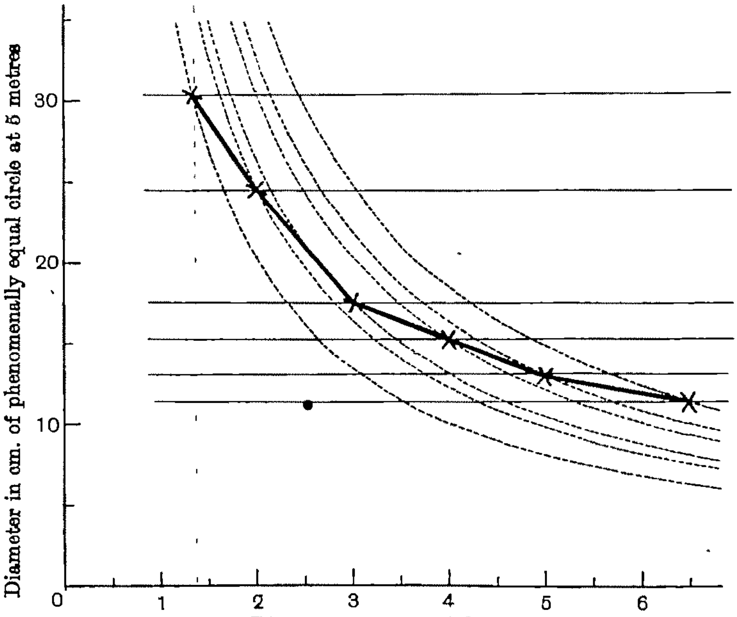

Fig. 6. Change of phenomenal size of circular disc with changing distance.

curve of decrease in phenomenal size is seen to fall all the way between these limits. Nowhere does the apparent size of the disc remain constant in spite of changing distance; nowhere does it change as rapidly as does the retinal image. The apparent change in apparent size is always a compromise between the change in stimulus size and the constancy of the 'real' size.

Köhler (4) has demonstrated that apes trained to react to the larger of two similar boxes continue to do so when its distance is so great that its perspective size is less than that of the other. Again, since he did not determine the distance at which the reaction was reversed (as from analogy with the above experiments we should expect it to be), no exact index of phenomenal regression can be determined. In Köhler's experiments R_1/R_2 (linear) was 4/3, while the greatest difference of stimulus sizes was when S_1/S_2 (linear) was 0·61. This shows that the index of regression was greater than 0·63. Since in my experiments, using discs of the same relative linear dimensions, most human subjects gave indices of regression greater than this, we cannot conclude whether or not the index of regression for chimpanzees is greater than that for human beings. A comparison of the results indicates that, at any rate, it is not much less.

IV. THE TENDENCY TO EQUALIZE VERTICAL SEMI-AXES IN PERCEPTION OF THE INCLINED SQUARE AND THE PHENOMENAL REGRESSION OF PARALLEL LINES.

In Section II, we considered only one respect in which the perception of the circle or square showed regression from the perspective to the physical figure—the ratio of the vertical to the horizontal axis. There is another character of the shape of the perspective figure which might also show phenomenal regression—the ratio of the upper to the lower semi-axis. In the perspective shape of an inclined square (Fig. 7), the

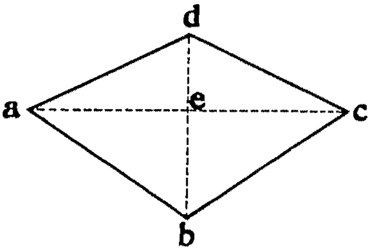

Fig. 7. Perspective shape of inclined square.

upper vertical semi-axis *de* is shorter than the lower one *eb*[1]; in the actual physical figure this ratio is unity. Phenomenal regression would, therefore, tend to make the ratio in the perceived or reproduced figure more close to unity than to its value in the perspective figure. Only a few observations were made on this matter. These indicated with certainty that this tendency to equalization of the vertical semi-axes is present in a large degree, although the number of observations was insufficient for exact measurement of its amount. Regression appeared to be more complete with respect to this character than in the character of equality of the ratios of the vertical and horizontal axes.

For this part of the investigation the square was necessarily used as object, since a reproduction of the circle gives no definite point from which the semi-axes can be measured. Also it was obviously more convenient to use the drawing rather than the matching method. The results of four experiments with the subject S. are shown in Table VII. It will be seen that for the nearest position of the object, this character showed the very large index of regression of 0·76, while for the other two positions it was not significantly different from unity, *i.e.* regression was apparently complete.

Table VII. *Tendency to equalize vertical semi-axes in drawing inclined square.*

Position of object	Ratio of semi-axes in perspective figure	Ratios of semi-axes in reproduced figure					Index of phenomenal regression
		1	2	3	4	Mean	
A	·69	0·96	0·95	0 88	0·875	0·915	0·76
B	·77	1 055	1·02	0·85	1·06	0·995	1·0
C	·82	0·97	1·02	1·115	0·92	1·005	1·0

It may be noted that this character is also an indication of the degree of convergence of opposite sides of the figure. The index of regression is a measure of the extent to which the converging lines of the perspective figure undergo regression in the phenomenal figure to the parallelism of the sides of the actual object.

There are other interesting consequences of this tendency of receding parallel lines to regress from the convergence of their stimulus character

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[1] Their relationship is given exactly by the equation

$$de/eb = (D - R \,.\, \cos\theta)/(D + R \,.\, \cos\theta),$$

where D is the distance of the subject's eyes from the centre of the square, R is half the diagonal of the square, and θ is the angle between the plane of the square and the line of vision. Fig. 7 is actually the perspective figure for the square in position A of Fig. 1, where this ratio was 0·69.

to the parallelism of the 'real' object. In some cases, this tendency may result in a phenomenal divergence. A striking example of this is to be found by looking through a telescope or through binoculars at a wall with parallel top and bottom receding from the observer. Under these conditions, the top and bottom of the wall appear to diverge considerably as they go from the observer. The explanation of this appears to be that when looked at in the ordinary way the wall appears to converge, but much less so than does the retinal stimulus, regression having taken place to the 'real' character of parallelism. When looked at through a magnifying instrument, the wall appears nearer and the amount of phenomenal regression which takes place is not that proper to its actual distance but the greater amount proper to its apparent distance, so that the regression is beyond parallelism to divergence. Another way of stating this is that regression is taking place towards the non-existent object with divergent sides which, if it occupied the apparent position of the wall as seen through binoculars, would cast on the retina an image with the degree of convergence of that actually formed (this degree being, of course, considerably less than that from a parallel-sided object in the same position).

A more convenient method of showing the same phenomenon is by an isometric or other parallel-sided projection of a cube (Fig. 8). If we see this as a solid figure with the corner *a* towards us (a mode of perception favoured by the fixation of *a*), the two pairs of edges perpendicular to the vertical edge through *a* appear to diverge. A similar effect is seen in the other two pairs of edges if the phenomenal figure is reversed and *b* is seen towards us.

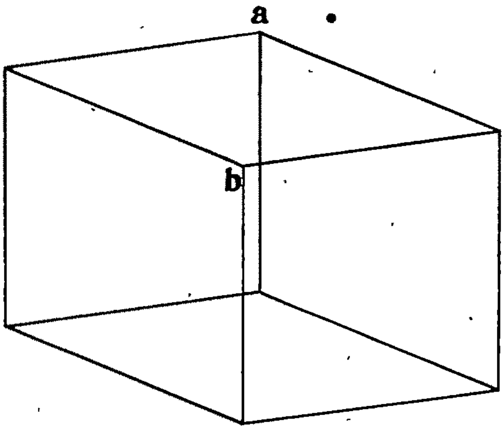

Fig. 8. Isometric projection of cube.

This may be explained by saying that this figure is a true perspective projection of an obliquely truncated pyramid with actually divergent sides[1]. This, however, is not a complete answer to the problem. If, as is commonly believed by writers on perception, the phenomenal object is entirely determined by the characters of the retinal projection, this figure should give a parallel-sided perception, whatever might be the shape of the object of which it is the projection. On the other hand, the phenomenal object is not a function only of the character of the object seen, since, if we look at an actual cube, the edges receding from us seem to converge somewhat. We can make a series of figures like Fig. 8 with the sides actually converging and select a member of the series in which the actual convergence exactly neutralizes the phenomenal divergence for one of the two ways of perceiving it. It is then seen as a parallel-sided figure. Similarly we can have a series of almost cubical solids with increasing actual divergence of sides and select one which appears parallel sided in perception. This would not be the one giving a parallel-sided retinal projection but one giving a retinal projection with sides still convergent but less so than that of the true cube. As with the perception of shapes, brightnesses and sizes, we are dealing with a compromise effect. The phenomenal character is a compromise between the character of the peripheral stimulus and that of the object (either 'real' or intuited).

V. SUMMARY.

Experiments were performed on the shapes of objects viewed obliquely, the apparent brightnesses of differently illuminated surfaces of different reflectivity, the apparent sizes of objects at different distances, and the apparent convergence of parallel lines receding from the observer. In all of these cases it was found that what was seen was intermediate between what was given in peripheral stimulation and the 'real' character of the object. To this effect of the character of the 'real' object on the phenomenal character we may give the name 'phenomenal regression to the real object.' We may use as measure of this effect the index

$$(\log P - \log S)/(\log R - \log S),$$

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[1] More precisely, of an infinite series of such solids of which the figure in which *a* and *b* are in the same plane (*i.e.* the projection itself) is a limiting member. There is the further psychological problem of why the phenomenal object should be a particular member of this series, and of why it should be so difficult to see the diagram as a plane figure with parallel sides since this is as much a member of the series of figures of which this could be a projection as any other. Possibly there is a tendency to approximate the phenomenon as nearly as possible to the cube.

in which P is a numerical measure of the phenomenal character, R of the 'real' character, and S of the stimulus character.

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IN WHAT SENSE CAN WE SPEAK OF PRIMARY COLOURS?

By JAMES DREVER.

(*From the George Combe Laboratory, Edinburgh University.*)

I. *Introduction* (pp. 360–361).
II. *Physical views* (pp. 361–363).
III. *Difficulties in the physical theories* (pp. 363–364).
IV. *Psychological criteria* (pp. 364–365).
V. *Peripheral colour vision and primary colours* (pp. 365–367).
VI. *Conclusion* (p. 367).

I. INTRODUCTION.

AT the present time it is mainly, though not exclusively, the physicist who speaks of 'primary colours.' The German equivalent 'Grundfarben'—in some respects a preferable term—is used, it is true, in the formulation and discussion of the Helmholtz theory of colour-vision by all and sundry, but in these discussions the physicist's point of view is still for the most part predominant. There is also, however, an entirely different point of view from which, and universe of discourse in which, both terms have been used—pictorial art. The colours designated 'primary' by the artist differ from those designated 'primary' by the physicist. The artist's primaries are red, yellow, and blue, the physicist's red, green, and blue. The difference is due to the difference between the two groups of phenomena, to which what is essentially the same logical principle of classification is applied. The artist finds that all the known bright colours can be derived from a mixture of red, yellow, and blue pigments. Vermilion and chrome-yellow give him orange; chrome-yellow and ultramarine green; vermilion and ultramarine purple, and so on. Arguing in the same way of spectral lights, the physicist claims that all the colours of the spectrum and of nature can be produced by the admixture in suitable proportions of red, green, and blue lights, but not by mixing red, yellow, and blue lights. He claims—and apparently with justice—that his point of view is more fundamental than that of the artist, that it is scientific and not merely empirical, that the artist's results in mixing pigments can be readily explained from this more fundamental point of view, in fine that in a scientific sense his primaries must supersede the primaries of the artist. The question which the present paper aims at discussing is whether we really have a scientific and ultimate basis for the classification of the colours in the primaries of the physicist, and its general purpose is to show that formidable difficulties, and what appear to be striking inconsistencies with verifiable facts, are involved in the claims of the physicist. It seems indeed that the last word in this matter does not rest with the physicist at all, that as far as colour is concerned there is a still more fundamental point of view, and that the logical principle of classification which both artist and physicist have adopted is itself not above suspicion.

II. PHYSICAL VIEWS.

When Newton decomposed white light into its elementary components he sought to distinguish seven such components—red, orange, yellow, green, blue, indigo, and violet—and he represented them diagrammatically in the form of a circle, the completion of the circle being made by the addition of the purples between red and violet. The choice of the number seven was undoubtedly determined by the mystical value attaching to that number, and the same mysticism underlay the selection of indigo as an elementary colour, the motive in this case being to work out an analogy between the notes in the octave and the spectral colours—the music of the spheres. There is possibly also some unconscious mysticism underlying the later selection of the number three. In any case there is no reason at this point why the physicist as physicist should select any particular colours in the spectrum as elementary. Regarded physically, and with reference to the decomposition by a prism of white light into its components, the elementary spectral colours ought to be infinite in number.

The decomposition of white light into the spectral colours, together with the recombination of the spectral colours into white light, may be taken as the fundamental factual basis for a physical theory of colour, but, as Goethe pointed out long ago, not necessarily for a physiological or psychological theory. A second fundamental fact, and the fact upon which the mathematical treatment of the physical data—which we shall come to presently—is based, is the fact that by combining two spectral colours, not too far apart in the spectrum, we can, by suitably varying the proportions in which we combine them, produce all the intermediate colours. In this connection another very important point emerges. Physically regarded, the colour produced by mixing two homogeneous colours—say red and yellow—is fundamentally different from the intermediate spectral colour which appears to the eye identical with it. When the mixture of lights is passed through a prism it is separated into the two bundles of rays with which we started, whereas in the case of the spectral colour, with which the mixture appears identical, no such result is produced by passing through a prism. Physically, therefore, the statement that we can produce a spectral colour by mixing two other spectral colours is contrary to fact. Psychologically and physiologically—though the latter is by no means certain—the mixture may be the same as the homogeneous colour, but physically the two are different. This very important fact seems to be entirely ignored in the mathematical treatment by the physicist of the phenomena of colour, which leads ultimately to the defining of the three 'primary' colours.

This mathematical treatment begins with the "Centre of Gravity Law of Colour Mixing," which was originally formulated by Newton. If we take two points A and B, not too far apart in the spectrum, then all the colours obtained by mixing the colours at A and B may be represented by a straight line AB, and any colour C, obtained by mixing an amount m of the colour A with an amount n of the colour B, is represented by the point C, the position of which is determined by the relation $AC : CB = m : n$ (Fig. 1). If now we take three colours in the spectrum, no one of which is producible by a mixture of the other two, then any colour p in the spectrum can be expressed by an equation of the form $p = xA + yB + zC$, where A, B, and C are the colours chosen in accordance with this principle. This is the second step in the mathematical treatment, and it permits us to represent all the colours of the spectrum geometrically by a triangle in place of Newton's circle.

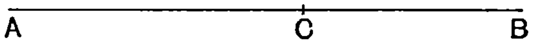

Fig. 1.

But the three colours A, B, and C are still undefined. Theoretically indeed there may be an infinite number of groups of three colours which satisfy the conditions that no one is producible by a mixture of the other two, and that any colour p can be produced by mixing them in suitable proportions. For, if we take a new group of three colours, A', B', C', satisfying the first condition, it is clear that each of these can be expressed in terms of our original A, B, and C, and consequently p is expressible in terms of the new group by an equation of the form $p = x'A' + y'B' + z'C'$. Our primaries, therefore, or the vertices of our colour triangle, have still to be defined. It has been suggested—by Abney, for example—that this difficulty is surmounted by adding to our original conditions the condition that no one of the three primaries is producible by a mixture of any two colours in the spectrum. But there is no colour in the spectrum that satisfies this condition in an absolute sense, *i.e.* if we neglect saturation, or, alternatively, if we take saturation into account, probably every colour in the spectrum satisfies it. The colours ordinarily recognized, however, as red, green, and blue—note the psychological criterion—cannot be produced in a high saturation by the mixture of two other colours. Accordingly these three colours are to be selected as the three primaries.

III. DIFFICULTIES IN THE PHYSICAL THEORIES.

New difficulties promptly make their appearance. Consider first the colour triangle as ordinarily represented (Fig. 2). If we place at the three angles the red, green, and blue, which mixed in equal quantities give us white, we have an equilateral triangle, the sides of which represent respectively the colours of the spectrum from red to green, from green to blue, and from blue to red through violet and the non-spectral purples, while the centre of gravity of the figure—the centroid—represents white, and the different parts of the surface all the various hues of diminished saturation between white and the various spectral colours. It is very easy to show how inadequately the phenomena of colour are represented by this colour triangle. If the triangle is equilateral pre-

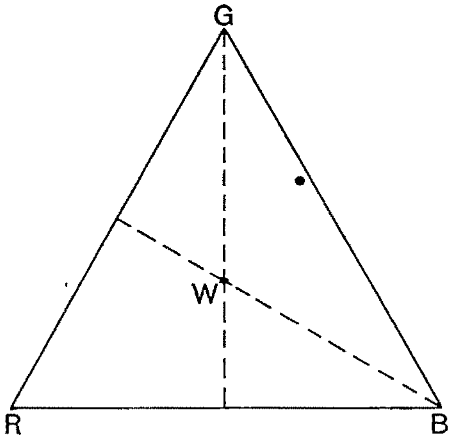

Fig. 2.

sumably the same number of distinguishable hues are to be found between *R* (red) and *G* (green), *G* (green) and *B* (blue), and *B* (blue) and *R* (red), which is contrary to fact. Moreover the colours nearest to $\bar{W}$ (white), and all equidistant from it, will be an orange-yellow, a blue-green, and a purple, which is also contrary to fact.

It is not necessary to go into the various attempts that have been made to meet these and similar difficulties. At the best they represent a compromise between the contentions of physiologist and psychologist, and the needs of the physicist, and the compromise can hardly satisfy either. It involves regarding the three primaries at the vertices of the triangle as mere points of reference, and at the same time colours "that never were on sea or land."

There is one other point that must be noticed before leaving the three primary colours of the physicist. Whether we take them as red, green, and blue, as is usually done, or as red, green, and violet, as many physicists prefer, it is obvious that scientific purposes will not be served unless we can define them more accurately than by the use of these popular and indefinite colour names. This more accurate definition has been attempted and with somewhat interesting results. König, for example, specifies as the three primaries a red complementary to $494\mu\mu$, but itself not in the spectrum, a green at $505\mu\mu$, and a blue at $470\mu\mu$. König's red, if we may so call it, is a mixture, or equivalent to a mixture, of spectral red with violet (or blue), which is of course inconsistent with the condition that no one of the three primaries can be produced by a mixture of spectral colours, and is not red psychologically. Reviewing the whole history of the three primary colours in physical theory, one cannot but feel that the colours originally chosen were not based on objective data, but, as already pointed out, were really those which appeared psychologically pure, or simple, but in order that objective criteria might be satisfied colours were finally chosen, which no longer satisfied the original psychological conditions. Tschermak, in a recent number of *Die Naturwissenschaften*[1], points out that the physicist's three primaries have invariably involved four physiological and psychological primaries, and this seems to be the case.

IV. PSYCHOLOGICAL CRITERIA.

This situation naturally raises the question whether there are reliable psychological criteria, which will enable us to determine with some degree of accuracy the fundamental or primary colours, in the sense of those colours which are pure or simple, as distinct from those which look as if they were mixed colours. It ought to be possible to settle this question experimentally. In a preliminary series of experiments in the George Combe Laboratory at Edinburgh, two subjects with good colour-vision were tested with the Hue Discrimination Spectrometer. Each was asked to set the drum at a position at which the colour in the field seemed pure—red, yellow, green, or blue, as the case might be. The results obtained were as follows:

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[1] Jahrgang XVIII, Heft 26, 27 June, 1930.

Red—average setting for both subjects 650μμ, with a range of variation with the first subject of 30μμ, and with the second subject of 15μμ.

Yellow—average setting with the first subject 562μμ, with a range of variation of 40μμ, and with the second subject 569μμ, with a range of 30μμ.

Green—average setting with the first subject 505μμ, with a range of 30μμ, and with the second subject 512μμ, with a range of 20μμ.

Blue—average setting for both subjects 472μμ, with a range of variation with the first subject of 4μμ, and with the second subject of 1μμ. (See also table below.)

The agreement for green and blue with König's figures is rather striking, but in all cases except blue the range of variation is much too great. Apparently, therefore, we shall have to admit the impossibility of arriving at a sufficiently stable determination of the psychologically pure or simple colours.

The position then would seem to be that, while the psychological criterion is to be preferred to the physical, and, that being so, the simple character of yellow is not for a moment in doubt, yet the variability and possibly more or less accidental character—conceivable dependence on the nature of the terrestrial environment—of the simple or primary colours determined on a psychological basis suggest the need of a more stable and objective criterion, if we are to recognize primary colours at all. Is there any characteristic of our organ of vision which can give us a basis for the classification of colours into primary and secondary, or simple and compound? When we ask this question the phenomena of perimetry at once suggest themselves as phenomena in which we may find an answer.

V. PERIPHERAL COLOUR VISION AND PRIMARY COLOURS.

The exploration of the peripheral retina had its beginnings in the work of Thomas Young, published as early as 1801. Young, however, was interested merely in the problem of the sensitivity of the marginal retina to light, and does not seem to have hit upon the colour phenomena. The different effects of different colour stimuli on the marginal retina were first investigated by Purkinje some twenty years later. Purkinje discovered that all light stimuli were colourless in the peripheral zone, that some stimuli showed change of colour between this zone and the position at which the true colours appeared, and that other stimuli did not show any colour change. Purkinje in fact discovered all the main phenomena, but he confessed himself unable to suggest an explanation. In 1840 Szokalsky, from results similar to those of Purkinje, drew the inference that the retina is composed of three concentric zones, the outermost zone being sensitive to black and white, the intermediate zone to black, white, blue, and yellow, and the innermost zone to those with red. Subsequent investigators added little of significance from our present point of view, though it is interesting to find Raehlmann and also Krükow in 1873 becoming aware of the difficulty of reconciling with the Young-Helmholtz theory the fact that yellow is sensed in parts of the retina where there is no sensing of red or green.

During the last two decades of the nineteenth century a series of investigations by Bull, Hess, and Hegg brought to light facts which are crucial for our present problem. The main object of these investigations was to delimit more clearly the colour zones in the retina, but in all cases one of the preliminary problems was to determine the stable colours ('unveränderlich' was the term used by Hess). In all cases such colours were found, and there were four. Hess gave as the wave-lengths of these stable colours: yellow $574{\cdot}5\mu\mu$, green $495\mu\mu$, blue $471\mu\mu$, the stable red being outside the spectrum and identical with König's red. In all these investigations coloured papers were used. In 1905, however, Baird published the results of experiments with coloured lights, which definitely confirmed the conclusions of Bull, Hess, and Hegg. Spectrometric analysis of the lights gave as the stable colours: yellow from $551\mu\mu$ to $587\mu\mu$, green from $483\mu\mu$ to $500\mu\mu$, blue from $448\mu\mu$ to $474\mu\mu$, red being again König's red beyond the visible spectrum. These results were confirmed by Dreher in 1911.

It seems, therefore, that four physiologically stable colours can be determined by perimetry. Colour changes with variation in intensity of illumination and with fatigue yield results in close agreement. The following table gives a selection of the results obtained by different investigators, so that agreements and differences may be seen at a glance:

	Red	Yellow	Green	Blue
Konig	Compl. to 494	—	505	470
Exner	,,	—	508	475
Goldmann	,,	568	504·5	468
Schubert	,,	575	503	468
Hess	,,	574·5	495	471
Baird	,,	569	491·5	461 (average)
Edin. 1st subj.	650	562	505	472
Edin. 2nd subj.	650	569	512	472

The physiologically stable blue and yellow agree fairly closely with the psychologically pure blue and yellow. On the other hand, the physiologically stable red and green do not look psychologically pure colours—both look bluish. Moreover, the physiologically stable red coincides with König's red, while the physiologically stable green is its complementary, and the psychologically pure green coincides with König's green. It is possible that the discrepancy between physiological and psychological red and green may be due to macular pigmentation, and to the fact that our standard light—that is, normal daylight—is yellowish. However that may be the discrepancy remains.

VI. CONCLUSION.

How are we then to answer the question with which we started? It is clear that the final decision does not rest with the physicist. Colour phenomena are physiological and psychological phenomena—in the last resort psychological. Psychologically there seem to be four simple or pure colours, and no more than four. Physiologically also there seem to be four stable colours and no more than four. But in respect of two out of the four psychologist and physiologist are not in agreement. Under the circumstances it appears to be the best plan to abandon the use of the word 'primary' altogether, except possibly in the artistic sense, and to speak of 'physiologically stable' and 'psychologically simple' colours, recognizing four in both cases. The number three has no rational basis, but probably, as we have seen, a mystical. In any intelligible sense of 'primary,' if we are to use the term at all, the physiologist has good grounds for maintaining that there are only two 'primary' colours—blue and yellow. Psychologically there are undoubtedly four.

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CHARACTERISTICS OF PROOF-READING.

By M. D. VERNON.

(*From the Cambridge Psychological Laboratory.*)

I. *Introduction* (pp. 368–370).
II. *Method of experiment* (pp. 370–371).
III. *Results of the experiments* (pp. 371–378).
IV. *Conclusions* (pp. 378–380).
V. *Summary* (pp. 380–381).
Appendix (p. 381).

I. INTRODUCTION.

It has been abundantly proved in the past that when we read we do not perceive separately and individually every letter of every word of the text. Our speed of reading is such that we cannot gain more than a vague general impression of the word, and possibly of some of its more prominent letters. The proof-reader, on the other hand, must perceive every letter of every word with sufficient clearness to detect even minute errors and misprints. What is it, then, which differentiates the reading of the proof-reader from normal reading? And are any special abilities or characteristics necessary for the efficient proof-reader? It seems probable that the proof-reader's perception of the words and letters is of the subliminal type which does not reach consciousness as long as it does not vary from the normal and habitual, but only when it is of an unusual nature. Thus it would be similar to the awareness of the cessation of a long-continued noise which had previously ceased to reach consciousness. But the normal reader is not incapable of this type of perception. Anyone who has attempted to read proofs will have been conscious of adopting an attitude of alertness and special receptiveness to any unusual feature of the printed material, that is to say, to any misprints. It seems possible, then, that the proof-reader may be able habitually to adopt a perfected form of this attitude when reading proofs. But we do not know what if any innate mental or physiological capacities are necessary, or at least favourable, to its inception. Nor can we assume that the proof-reader does not possess some specialized faculty or mode of procedure which enables him to detect misprints with an accuracy impossible to the ordinary reader.

Crossland (1) concluded from his work on proof-reading that some form of innate ability did exist. He compared the rate of reading of proof-readers and their ability to detect misprints with those of readers untrained in proof-reading. He found that the trained readers as a group read more slowly than the untrained, and deduced that training had probably lengthened the fixation pauses during reading. But although on the average the trained proof-readers showed more accuracy in detecting misprints than the other readers, several of the latter were quite as accurate as the former, and some were more accurate. Thus the longer fixation pauses and slower reading times did not necessarily lead to more accurate perception. Also, when the frequency of the misprints in the proof was varied, on the whole the accuracy of the trained proof-readers varied less than that of the untrained; but it was found that, taking all the readers together, the most accurate ones were the least variable, rather than the trained ones. Thus Crossland concluded that some underlying ability for keen perceptual discrimination determined both the accuracy in detecting misprints, and the variability of that accuracy with the frequency of errors. Clearly some trait or mental 'set' enabled the accurate reader to detect errors with skill; but it is not easy to determine the nature of this trait. It appeared that the aim or object of the reader was a factor of some importance, since accuracy was much greater when special instructions for attention to misprints were given. Again, absorption in the meaning of the content seemed to decrease accuracy considerably. Since this occurred both with the trained and the untrained readers, Crossland considered that ability to ignore this meaning was not acquired with practice, and was probably innate when it did exist. It was found by Downey (2) that when sentences containing misprints were exposed tachistoscopically, the most intelligent subjects at first ignored the misprints, and read entirely for the meaning of the sentences, but when they realized the presence of the misprints, they quickly corrected themselves, and afterwards detected more misprints than the less intelligent subjects. From this Crossland deduced that general intelligence is correlated with proof-reading ability, and that the common factor may be innate ability for accurate perceptual discrimination, since this plays a considerable part in general intelligence as usually estimated. But the work of Whipple (6) and Gates (3) has shown that there is no such thing as general perceptual ability; the ability varies according to the nature of the stimulus object. And we may infer that the detection of misprints in ordinary proofs does not involve the same attitude or the same ability as does their tachistoscopic perception. The part played by the assimilation of the meaning and the general contextual setting is quite different in the two cases. Thus we may be led to suppose that proof-reading ability may be to some extent connected with the temperamental basis which must underlie the attitudes or aims that enable the proof-reader to concentrate upon the perception of misprints.

But the results of Crossland may equally show that certain individuals are able when called upon to assume an attitude or mental 'set' such that the general meaning of the content is ignored, while the recognition of misprints occupies the centre of consciousness. And it seems quite possible that only long training will enable the reader to maintain this attitude indefinitely, without undue effort and strain, though the untrained reader can do so for a short period, perhaps with considerable effort. A study of the underlying mental processes concomitant with reading should throw some light upon these attitudes. It has been shown by the writer (4) that the variations in the eye movements and fixation pauses which occur in normal reading are in general closely related to the underlying mental processes. It was thus thought that some information as to the nature of the methods and abilities of professional proof-readers might be obtained by studying their eye movements in reading normal material and material containing misprints, and comparing them with the eye movements of other individuals who were not professional proof-readers. Moreover, Crossland's theory as to the unusual length of the fixation pauses of proof-readers could be tested, and also the importance to the accurate proof-reader of natural accuracy of eye movement and fixation.

II. METHOD OF EXPERIMENT.

The method by which the eye movements were recorded has already been described in detail in this *Journal* (5). In brief, a beam of light is directed on to the cornea of the right eye, reflected by it and focussed to form a magnified image upon a photographic film which travels vertically at a steady rate. Fixation pauses appear as a series of fine vertical black lines upon the film. A time record is also photographed upon the film, so that the duration as well as the number of the fixations can be measured[1]. Eight passages, each ten lines in length, of normal printed reading matter, dealing with various topics, were read by each subject. In addition were read eight passages, each twenty lines in length, of reading matter of similar content and style; the second, third, sixth and seventh of these contained a number of misprints varying from nine to nineteen. The third passage is appended, as a specimen.

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[1] In practice it was found more convenient to arrive at the average duration of the fixations in the reading of each line of print by dividing the time taken to read the line by the number of fixations. The time taken by the inter-fixation movements was neglected, since it is small and regular.

It was shown in a former series of experiments (4) that the number and duration of the fixation pauses made in normal reading appeared to be related to the accuracy of voluntary eye movement from point to point, and the steadiness of voluntary fixation upon a point. Thus in this series also records were taken of the movements of the eyes to and fro between points situated at visual angles of 5°, 10° and 20° to the left and right of the central point; and also of long fixations for periods of twenty seconds of points situated at these angular distances from the central point.

The subjects of these experiments were (1) four proof-readers, called *W*, *X*, *Y* and *Z*, from the Cambridge University Press, (2) five of the subjects, here called *A*, *B*, *C*, *D* and *E*, who had taken part in the previous series of experiments (4). Three of the proof-readers were 'press' readers; that is to say, they read the final proof before it was printed for publication, and read it more or less straight through without a word-by-word comparison with the original MS. They had been proof-readers for thirty, sixteen and ten years respectively. The fourth was a young man who had been proof-reading for two or three years only; he was a 'first-proof reader'—that is, he read the first rough proof from the printer, comparing it continually with the author's MS. Thus he had not only had much less experience than the other three, but was also less accustomed to rely upon his own acumen and judgment in detecting and correcting misprints. One qualification must be attached to the results obtained from the first three subjects; their eyesight was not normal, and two of them were so short-sighted that it was necessary to place the smaller printed material closer to their eyes than the normal reading distance.

Of the other five subjects, four were graduates and research workers in psychology. The fifth had a considerable knowledge of psychology, but less general education and less practice in reading than the others.

III. RESULTS OF THE EXPERIMENTS.

In Table I are shown the average results for the nine subjects reading the twelve passages of normal reading material. It appears that the average time taken to read a line of print was slightly greater for the proof-readers as a group than for the other subjects; but the overlap was much too large to allow any conclusions to be based upon the difference. The standard deviations (S.D.), which measure the variation of the reading time from line to line, are, however, less for the proof-readers than for any of the other subjects except *B*. Again, although on the whole the proof-readers made fewer fixation pauses per line than the other subjects, there was an overlap; but the standard deviation of number of pauses was less for the proof-readers than for any other subject except *B*. The average duration of the pauses was much the same for proof-readers and others, and so also were the standard deviations of duration[1]. But the regressions[2] were much fewer for the proof-readers, except Subject *Z*, than for the others, except Subject *B*. Now it is clear that frequent regressions prevent a methodical and regular type of reading. Hence we may deduce that the proof-readers, in reading normal material, are not much slower than the ordinary reader, but are on the whole considerably more regular and methodical, and less variable. This conclusion is immediately apparent on viewing the photographic records of the fixations; the almost machine-like regularity of the succession of pauses shown by the proof-readers may be contrasted with the irregularity of the other subjects. Subject *Z* provides an instructive exception. His tendency to regress was very similar to that of the ordinary reader, and it is tempting to assume that it had not yet been eliminated by years of training.

Table I.

	Reading time per line in sec.		No. of pauses per line		Duration of pauses in sec.		No. of regressions per line
Subject	Av.	S.D. from Av.	Av.	S.D. from Av.	Av.	S.D. from Av.	
A	2·61	0·528	10·4	2·05	0·255	0·0375	1·23
B	1·76	0·332	7·4	1·33	0·242	0·0330	0·16
C	2·68	0·677	9·1	2·03	0·302	0·0400	1·08
D	2·23	0·498	9·6	2·02	0·231	0·0261	1·27
E	2·41	0·500	11·2	2·20	0·218	0·0258	2·04
Av.	2·34	0·507	9·5	1·93	0·250	0·0325	1·16
W	2·93	0·343	8·8	1·09	0·342	0·0452	0·49
X	1·95	0·272	8·9	1·21	0·216	0·0239	0·31
Y	3·11	0·459	11·1	1·68	0·281	0·0293	0·48
Z	1·98	0·317	8·6	1·37	0·228	0·0307	1·39
Av.	2·49	0·348	9·4	1·34	0·267	0·0323	0·67

But it was also pointed out that Subject *B* showed as little variation and as few regressions as did the proof-readers. This extreme regularity seems to have been due to the total lack of interest with which this subject read the material. In a former series of experiments (4) it was found that when he was interested in the content of what he was reading, and was attending closely to the meaning of the content, his variability and number of regressions were considerably increased, although they were still less than those of Subjects *A*, *C*, *D* and *E*. Thus it appeared that a reader with a natural tendency to regularity of reading might be even more regular when scarcely paying any attention to the meaning of the content. This connection between lack of attention and regularity of the reading processes was manifested fairly generally by all the non-proof-readers. This supports Crossland's conclusion, that one qualification for a proof-reader is to pay little attention to the general meaning of the content. The introspections given by one or two of the proof-readers lead to the same conclusion. They stated that it was necessary to assimilate to some extent the meaning of the words and phrases, in order to correct grammatical errors and also contradictions and disparities of statement. But there was little or no comprehension of the ultimate direction of the argument, or the general conclusions of the reasoning, and, in all probability, none of the associated thought and imagery which constitute the ultimate significance of words and phrases. Similarly the proof-readers knew the correct spelling and even the correct use of technical terms and phrases without understanding in the least what they meant. Hence it seems probable that the most accurate correction of a proof is made by a reader who is not personally interested in the topic under discussion; or by one who by long practice has learnt to inhibit any such interest.

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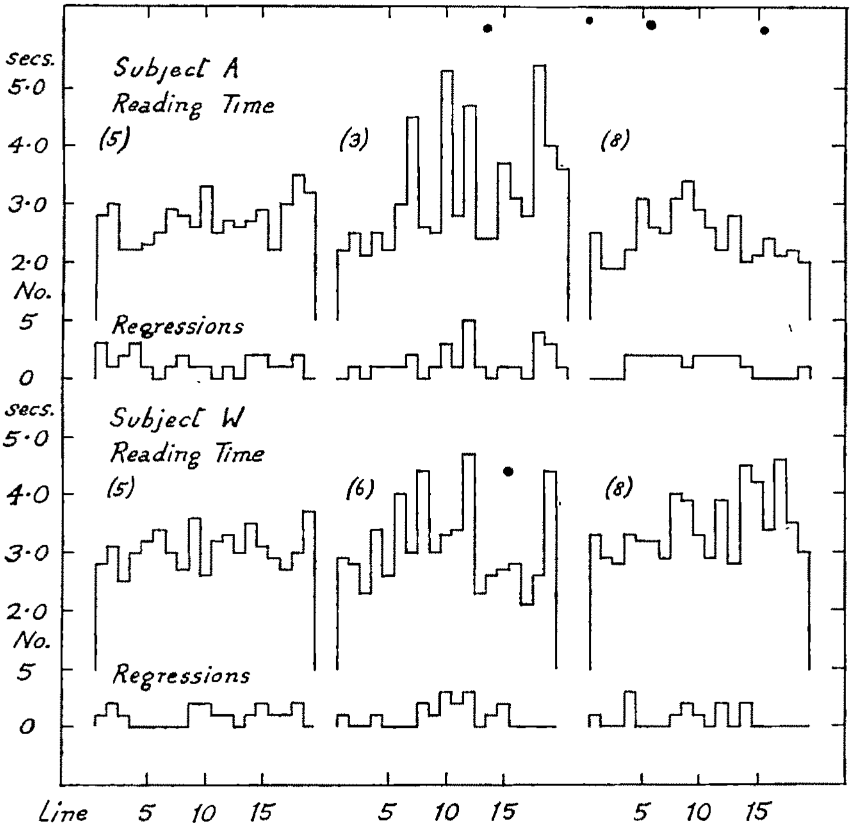
[1] Had it been possible to measure the duration of each fixation pause individually, group differences between the standard deviations might have been more apparent.

[2] A regression occurs when the eye moves backwards from the word fixated to another before it in the line, or in the previous line.

The same regularity of procedure appeared among the proof-readers when reading material containing errors. This procedure was not altogether comparable with actual proof-reading, because the readers were not able to stop and correct the errors. They often experienced a feeling of struggle and effort as a result of this, but stated that they were able to continue straightforward reading, without regression and without any difficulty in understanding the meaning of the content. The non-proof-readers, who experienced no habitual tendency to correct the misprints, were far more affected by them. After having noticed one or two obvious misprints, they usually adopted an alert, searching attitude—on the look-out for more misprints. This was frequently accompanied by pronounced affect—with feelings of irritation or amusement, or with heightened interest in the pursuit of 'spotting' the misprints. In consequence, interest in the meaning of the content usually diminished; and in one passage where there was a misprint in nearly every line, comprehension of the meaning was generally very vague, and sometimes completely absent. These attitudes were not infrequently carried over into the reading of the subsequent passages containing no misprints; but they usually disappeared during the latter half of the reading, once it had become clear that there were few if any misprints. Thus it appears that in the naïve subject anything unusual in the perceptual situation—the proof—arouses an attitude of scrutiny, often affectively toned, towards its minute details. But in the proof-reader this attitude has probably become habitual, and is only accompanied by affect when the normal processes are inhibited, that is to say, when the error is not immediately corrected.

These conclusions are supported by the variations in the eye movement processes which appeared in the reading of the material containing misprints. Thus in Table II it is shown that in general the difference of average reading time per line between the passages (1) and (5), containing no misprints, and passages (2), (3), (6) and (7), containing misprints, is considerably less for the proof-readers than for the other subjects; the same is true on the whole for the standard deviations of average reading time, and for the number of regressions. Thus the proof-readers were more regular in their normal reading than the other subjects, and much more regular in their reading of material containing misprints. This must not be attributed to the fact that the proof-readers were less quick and accurate in detecting the misprints than were the other subjects; the reverse was the case.

It also appeared that the slight increase of reading time and its standard deviation which did occur was more or less maintained by Subjects *W*, *X* and *Y* during the reading of passages (4) and (8), containing no misprints, showing that the attitude of alertness and scrutiny, once adopted in response to the occurrence of misprints, persisted to a greater degree with these subjects than with Subjects *A*, *B*, *D*, *E* or *Z*. This is also illustrated in the Figure, which shows the reading time for each line and the number of regressions for Subjects *A* and *W* reading passages (5), (3) and (8), and (5), (6) and (8) respectively. In the reading of passage (3) Subject *A*'s reading time and regressions suddenly increased and became irregular on his realizing the presence of the misprints; and decreased and became more regular in the latter half of passage (8), when he found that there were no misprints in this passage. Contrasted with this, the increase of reading time and regressions for Subject *W* reading passage (6) is slighter and more regular; but the irregularity persists all through the reading of passage (8). The number of regressions made by Subjects *W*, *X* and *Y* in reading passages (4) and (8) was not much greater than that made in reading passages (1) and (5), which perhaps shows that the regressions in passages (2), (3), (6) and (7) resulted from the inability to correct the misprints in the accustomed manner. That is to say, they were an artifact of the experimental situation, and were not natural to the habitual procedure adopted in proof-reading; they did not occur in passages (4) and (8), where there were no misprints, because there was no habitual tendency to perpetuate them.



It was suggested by Crossland (1) that in proof-reading the duration of the pauses might be considerably lengthened, but this is not shown in these results. It is true that there was a slight increase of average duration for both groups of subjects between passages (1) and (5), and passages (2), (3), (6) and (7), and that the proof-readers remained at the higher level while reading passages (4) and (8), while the other subjects returned to the previous average duration. This is shown in Table III.

Table II.

Subject	Av. reading time per line in sec.					Standard deviation of reading time					Av. number of regressions per line				
	Av. for passages			Differences between passages		Av. for passages			Differences between passages		Av. for passages			Differences between passages	
	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]
A	2·07	3·06	2·04	0·39	0·12	0·400	0·830	0·553	0·370	0·283	1·08	1·59	1·60	0·51	− 0·01
B	1·78	2·19	1·83	0·41	0·36	0·234	0·744	0·406	0·510	0·338	0·13	0·62	0·26	0·49	0·36
C	2·58	3·34	3·28	0·76	0·06	0·619	0·837	0·944	0·218	− 0·107	0·95	1·57	1·47	0·62	0·10
D	2·26	3·05	2·66	0·79	0·39	0·604	1·205	0·617	0·601	0·588	1·47	2·31	1·76	0·84	0·55
E	2·56	3·03	2·78	0·47	0·25	0·499	0·793	0·514	0·294	0·279	2·35	3·32	2·99	0·97	0·33
Av.				0·56	0·24				0·400	0·276				0·69	0·27
W	3·07	3·23	3·45	0·16	− 0·22	0·325	0·679	0·554	0·354	0·125	0·89	1·04	0·63	0·15	0·41
X	2·03	2·43	2·39	0·40	0·04	0·192	0·506	0·367	0·314	0·139	0·26	0·56	0·34	0·30	0·22
Y	2·89	3·46	3·64	0·57	− 0·18	0·431	0·558	0·539	0·127	0·019	0·40	0·55	0·44	0·15	0·11
Z	1·99	2·13	2·18	0·14	− 0·05	0·327	0·324	0·263	− 0·003	0·061	1·70	1·45	1·37	− 0·25	0·08
Av.				0·32	− 0·10				0·198	0·086				0·09	0·21

Table III.

Subject	Av. duration of pauses in sec.					Av. number of pauses per line				
	Av. for passages			Differences between passages		Av. for passages			Differences between passages	
	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]	(1), (5)	(2), (3), (6), (7)	(4), (8)	[(2), (3), (6), (7)] − [(1), (5)]	[(2), (3), (6), (7)] − [(4), (8)]
A	0·243	0·260	0·251	0·017	0·009	11·1	11·8	11·7	0·7	0·1
B	0·225	0·246	0·224	0·021	0·022	7·9	9·4	8·3	1·5	1·1
C	0·310	0·325	0·293	0·015	0·032	9·4	10·5	11·3	1·1	− 0·8
D	0·229	0·235	0·239	0·006	− 0·004	9·8	12·8	11·3	3·0	1·5
E	0·216	0·217	0·217	0·001	0·000	11·9	14·2	13·1	2·3	1·1
Av.				0·012	0·012				1·7	0·6
W	0·304	0·304	0·338	0·000	− 0·034	10·2	10·7	10·4	0·5	0·3
X	0·232	0·237	0·215	0·005	0·022	8·9	10·3	10·4	1·4	− 0·1
Y	0·276	0·293	0·277	0·017	0·016	10·5	11·9	13·1	1·4	− 1·2
Z	0·233	0·236	0·237	0·003	− 0·001	8·9	9·1	9·1	0·2	0
Av.				0·006	0·001				0·9	− 0·3

The same applied, however, to the average number of fixation pauses per line. Thus it appears that the presence of misprints caused an increase both of number and duration of fixation pauses, which was slightly though not significantly greater among the non-proof-readers than among the proof-readers, but which persisted for the proof-readers during the subsequent reading of material without misprints. The misprints did not set up any differentiating effect between number and duration of fixation pauses, but merely produced a general increase of reading time per line. Nor had the fixation pauses of the proof-readers become permanently lengthened by long practice in proof-reading; the range of pause duration was very similar for all the subjects in reading all kinds of material.

Thus we may conclude that there is little obvious difference in objective procedure between the trained proof-reader and the ordinary practised reader. But clearly the proof-reader is more regular in reading normal material and still more in reading material containing misprints; his reading time and number of fixations are less variable, and he makes very many fewer regressions. He is able to deal with material riddled with misprints, even when these are unexpected, in a systematic and methodical fashion, without getting confused or allowing his eyes to wander wildly backwards and forwards.

If the only characteristic of the proof-reader's eye movements which differentiates them from those of the non-proof-reader is their much greater regularity, it is interesting to discover whether there are any characteristics of voluntary eye movement which differentiate the proof-reader. It was found that all readers tended to let their eyes fluctuate and wander away from the fixation point both during the long fixation periods of twenty seconds, and also during the short fixation periods between voluntary movements. It was difficult to measure the degree of fluctuation in these cases. But a rough estimate is provided by counting the number of fluctuations which could be detected. In Table IV these numbers are given for each subject as a percentage of the average for all subjects. It will be seen that on the whole the proof-readers made fewer fluctuations than the other subjects. The difference was more clearly marked in the short fixation periods than in the long ones; and in the latter, Subject *W*'s fixations were quite exceptionally fluctuating. This inability to fixate may have been due to ocular defect, since it was most pronounced. Table IV also shows that the proof-readers were among the more accurate in moving their eyes from one point to another—that is to say, they did not show so strong a tendency to stop short of or go beyond the correct point, and then make corrective movements. If the subjects are ranked in order from the steadiest and most accurate to the least steady and most inaccurate in voluntary fixation and movements, it appears that the average ranks of the proof-readers were higher than the average ranks of all the other subjects except *C* (see Table IV). That is to say, there was nothing phenomenal or unusual about the accuracy and steadiness of the proof-readers' voluntary movement and fixation; but they did tend on the whole to be among the steadiest and most accurate.

Table IV.

Subject	No. of fluctuations as per cent. of av. no. of		No. of inaccurate fixations per ten movements	Av. inaccuracy of movement in minutes of angle	Rank of subject in				Av. rank of subject
					No. of fluctuations in				
	Long fixations	Short fixations			Long fixations	Short fixations	No. of inaccurate fixations	Av. inaccuracy of movement	
A	125	138	8·2	53·3	7	9	7	6	7·3
B	91	110	8·1	55·7	5	6	4	8	5·8
C	58	94	6·9	40·7	3	5	1	3	3·0
D	107	136	8·8	54·6	6	8	8	7	7·3
E	163	125	8·9	55·7	8	7	9	8	8·0
Av.	108·8	120·6	8·2	52·0	5·8	7·0	5·8	6·4	6·3
W	198	78	7·5	32·1	9	3	3	1	4·0
X	25	85	8·1	47·1	1	4	4	4	3·3
Y	82	62	8·1	52·5	4	1	4	5	3·5
Z	47	67	7·0	38·6	2	2	2	2	2·0
Av.	88·0	73·0	7·7	42·6	4·0	2·5	3·3	3·0	3·2

IV. CONCLUSIONS.

The general conclusions seem, then, to be that good proof-readers possess a fair amount of accuracy and steadiness of eye movement, which is presumably innate because they had had no practice in making voluntary eye movements and fixations such as those just mentioned. This is coupled with considerable regularity of eye movement in reading; but it is not possible to say exactly how far this is natural or acquired. It is not acquired from much practice in normal reading; this is shown particularly by Subjects *C*, *D* and *E*, who had all had a great deal of practice in reading, but showed very variable reading times and many regressions. On the other hand, it does seem possible that regularity is acquired by much practice in proof-reading; since Subject *Z*, who had only been proof-reading a short time, made a good many more regressions than Subjects *W*, *X* and *Y*. It is probable that the regularity was only in part a habitual motor tendency. Both with the normal reading material, and the material containing misprints, it resulted to some extent from lack of interest in, or apprehension of, the general meaning of the content, as did Subject *B*'s regularity in reading the normal material. This brings us back to the theory originally propounded, that ability in proof-reading is largely a matter of attitude or mental 'set' towards the material read; and that, while other readers can assume it to some extent, the professional proof-reader has perfected it by long practice, and can assume and maintain it automatically. Thus one proof-reader stated that in ordinary reading for pleasure he might or might not notice any misprints in the text. But when proof-reading he felt quite different; he "got down to it," and was confident that he did not pass over any misprint. The proof-readers had not, of course, had any practice in introspection, and were unable to describe this mental 'set' at all clearly. It seems probable that it involved a species of 'side-tracking' of assimilation of the general meaning of the content, coupled with alertness and scrutiny of typographical detail. That is to say, the words and letters were perceived more as a series of designs (just as, in glancing at a book of Euclid, one might perceive and name 'triangle, circle, right-angle,' and so on); but the symbolic associations of the words were of secondary importance in consciousness, while the whole complex of associated thought and imagery which usually follows word perception was almost entirely inhibited. This method of reacting to the printed words had become almost as habitual to the proof-reader as had the converse method to the mature and practised normal reader.

It may, perhaps, be objected that there is little experimental basis for such a theory. It was shown, however, that the proof-reader reacted to material containing misprints with a type of objective behaviour which was much more systematic and methodical than that of the ordinary reader; and that this type of behaviour, once initiated, persisted over a period when no misprints were encountered—that is to say, it was relatively permanent, and not quickly abandoned. Moreover, the subsequent introspective reports seemed to show that the proof-readers were much less muddled and upset by unexpectedly meeting with the misprints. They were able to carry out the rudimentary assimilation of the meaning which, it was noted above, was necessary for the detection of errors of grammar, contradictory statements and so on; whereas the ordinary readers had very little idea of any meaning at all when the number of misprints was large. Thus it is clear that the proof-reader possessed a well-adjusted habitual mode of response to the presence of misprints; and this mode did not show any objective characteristics markedly different from those of normal reading. It is also reasonable to suppose that this mode of response involved a withdrawal of interest and attention from the meaning of the content. This was shown both by the fragmentary introspections of the proof-readers themselves, and by deduction from the fuller introspections of Subject *B*, whose procedure in reading normal material was so very similar to that of the proof-readers. Thus we may conclude that the mode of response to the mis-printed proof consisted of some such attitude or mental 'set' as that described above. It is still impossible to determine the extent to which this attitude is based upon underlying general intelligence or temperamental disposition. It seems fairly clear that there is no specific proof-reading ability, but that this aptitude must be related to a number of broad general traits of character. It also seems probable that natural accuracy of voluntary eye movement may be of assistance to the regular movement processes characteristic of the proof-reader.

V. SUMMARY.

1. The eye movements made by four proof-readers in reading normal material and material containing misprints were compared with those of five other readers who had had no training in proof-reading.

2. The average reading time per line and the number and duration of the fixation pauses of the proof-readers were much the same as those of the other subjects in normal reading; but the standard deviations of reading time and number of pauses and the number of regressions of the former were considerably less than those of the latter.

3. In reading material containing misprints, the reading time per line and the number of regressions increased much less for the proof-readers than for the other subjects, but this increase was maintained by the proof-readers, and not by the other subjects, during the subsequent reading of material containing no misprints.

4. It was concluded that the proof-readers were able to adopt and maintain indefinitely an attitude or mental 'set' towards reading material containing misprints such that the recognition of small details of the structure of letters and words occupied the centre of consciousness, while assimilation of the general meaning of the content was relegated to the background. This accounted for the regularity of their eye movements. Non-proof-readers could not adopt this attitude with the same efficiency and permanency.

5. The proof-readers were naturally fairly accurate and steady in the execution of voluntary movements and fixations.

In conclusion I wish to express my thanks to the Cambridge University Press for its great kindness in allowing four proof-readers to act as the subjects of these experiments. My acknowledgments are due to the Medical Research Council, by whose auspices this work was made possible.

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APPENDIX.

The whole descent is like a dream to me, so rapidly was it accomplished. I had scarcely left the summit ere the valley had closed round my path, and the sun beat upon me, walking in stagnant lowland atmosphere. I was in different country from the day before; The stony skeleton of the world was here vigorously displayed to sun and air. The slopes were steep and changeful. Oak-trees clung along the hills, well grown, weathy in leaf, and touched by autumn with strong and luminous colours. Here and there another stream would fall in from the right or the left, down a gorgeof snow-white and tumultuary boulders. The river in the bottom (for it was rapidly growing a river, collecting on all hands as it trotted on its way) here foamed a white in desperate rapids, and there lay in pools of the most enchanting sea-green shot with watery browns. As far as I have gone, I have never seen a river of so delicate and change ful a hue; crystal was not more clear, the meadows were not by half so green; and at every pool I saw I felt a longing to be out of these hot, dusty, and material garments, and bathe my naked body in the mountain air and water. all the time as I went on I never forgot it was the Sabbath; the stillness was a perpetual reminder; and I heard in spirit the churchbells clamouring all over Europe, and the psalms of a thousand churches.

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AN EYE FACTOR AFFECTING PROFICIENCY AT BALL GAMES.

By H. BANISTER and J. M. BLACKBURN.

(*From the Cambridge Psychological Laboratory.*)

I. *The 'good eye' not a true eye factor* (p. 382).
II. *True eye factors are possible* (p. 382).
III. *One such factor the inter-pupillary distance* (pp. 382–383).
IV. *The records* (pp. 383–384).
V. *Summary* (p. 384).

I. THE 'GOOD EYE' NOT A TRUE EYE FACTOR.

A 'GOOD EYE' is usually considered to be all important for proficiency at such games as polo, tennis, cricket, fives, etc., in which a fast-moving ball has to be hit with speed and precision either by the hand itself or by an instrument held in the hand. But, provided the individual can see, the 'good eye' appears to be quite independent of visual acuity. Many of the best players have an acuity far below the normal. In all probability the 'good eye' is not a true eye factor at all. It seems rather to be a very high innate visuo-muscular co-ordination, which enables the one who possesses it to hit the ball with his racket or bat held so that the plane of the face makes a particular angle with the direction in which the ball is travelling, the angle being determined by the way in which the ball is moving, and it enables him to hit the ball in exactly the right position in space and at the correct speed, with the time judged to an extraordinary degree of nicety. As is well known, all this may be accomplished by a man with very poor (uncorrected) visual acuity.

II. TRUE EYE FACTORS ARE POSSIBLE.

Yet it seems strange if no lower order visual factor than that entailed in this 'good eye' co-ordination is of importance: and much time has been spent in the attempt to find such elementary factors.

III. ONE SUCH FACTOR THE INTER-PUPILLARY DISTANCE.

So far only one of the elementary factors tested by us has given positive results, viz. the inter-pupillary distance (I.P.D.). The probable reason why this distance is of importance is that the wider apart the eyes the greater will be the differences in the disparation on the two retinae, with a consequent increase in the ability to judge the relative distances of objects. The individual whose eyes are close together approximates to the one-eyed man whose comparative inability to judge relative distances is well known.

IV. THE RECORDS.

The I.P.D.'s of 258 Cambridge undergraduates were measured to the nearest millimetre by means of a Zeiss Augenabstandsmesser, and each man was classified according to his ability at games. As always, this classification was most difficult. To obtain it each man entered on a prepared form his name, college, length of residence at Cambridge, and school last attended; he also stated his achievements, both at school and at the University, in the various games and pastimes. These statements were then gone through by a committee of four undergraduates, who decided into which of four classes each individual should be placed. The 1929 classification was made by each judge separately, without any reference to the other judges and the results were pooled, more weight being given to the judgments of those who knew the men personally than to the judgments of those who were going by the paper records only. The 1930 classification was made by the judges in collaboration.

Such classifications are manifestly inaccurate, but they are probably as accurate as can be obtained. They are inaccurate because the judges had never seen many of the men play, and even when they had seen them play their judgments were subject to the usual bias and 'halo' effects. Also good players in large colleges such as Trinity and St John's often experience difficulty in getting into their college sides, and so are neither known nor have any achievement to record at the University, whereas much poorer players from the smaller colleges find it difficult to escape from playing games, even if they so wish, and so tend to be put into a higher grade. In addition, a large number of men row, while others run or walk for exercise because they find that their studies are in that way less liable to interruption than if they play organized games. Such men, though they may be potentially good players of ball games must, according to these methods of classification, be ranked low.

Difficulties of these kinds are inherent in all enquiries into relations between attributes where the classification for one attribute has to be assessed, and militate against any relation being found.

The I.P.D.'s varied from 57 to 73 mm., the mean being about 64·5 mm.

Since the distances were measured to the nearest millimetre only, those men with an I.P.D. of 65 mm. or over were classified as above the average, those with 64 mm. or under as below the average.

The classification of these men in two groups, as Poor or Good players was:

I.P.D. (mm.)	Games ability: Poor	Games ability: Good	Total
up to 64	78	44	122
65 and over	67	69	136
Total	145	113	258

Statical analysis according to the χ^2 method shows that the probability of the distribution being due to chance is 1·8 per cent. In other words, the odds are 55 : 1 *against* chance.

Looked at from another point of view one may ask whether the I.P.D. of the Good player is greater than that of the Poor player on the average; and, if so, whether the difference is statistically significant. The answers to both these questions are in the affirmative. The mean I.P.D. of the 113 Good players is 64·946 mm.; that of the 145 Poor players is 64·157 mm. This gives a difference of 0·78 mm. on the average between the two groups. The standard error of the differences is 0·3517 mm., so that the difference of the means/standard error of the difference of the means = 2·24. Put in another way this means that the odds *against* the difference being due to chance are 39 : 1.

It therefore appears that one factor, though a small one, in goodness at ball games is the inter-pupillary distance.

V. SUMMARY.

The inter-pupillary distances of 258 Cambridge undergraduates have been measured, and the measurements have been compared with their goodness at ball games. Analysis of the results shows that those with the greater distance between the pupils are on the whole the better players. This is probably due to the better stereoscopic vision which the greater width makes possible.

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INDIVIDUAL DIFFERENCES IN THE PERFORMANCE OF A SIMPLE TEST.

By J. M. BLACKBURN.

(*From the Cambridge Psychological Laboratory.*)

I. *Introduction* (p. 385).
II. *Description of experiment* (pp. 386–387).
III. *Discussion of the mistakes* (pp. 387–390).
IV. *Discussion of methods of marking* (pp. 390–392).
V. *Conclusion* (p. 392).
VI. *Summary* (p. 393).

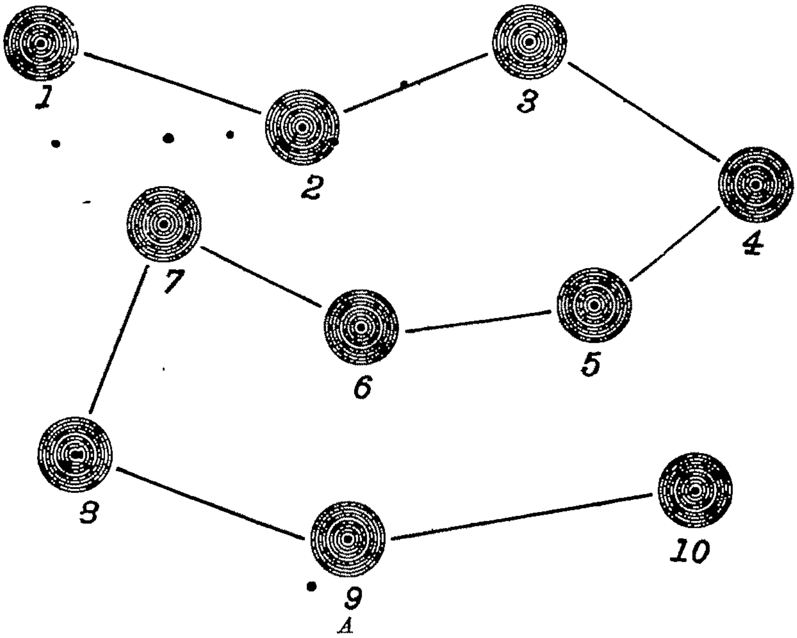
I. INTRODUCTION.

It is the aim of many psychologists to obtain psychological tests that will lend themselves satisfactorily to purely objective methods of marking, and tests to discover the presence or the absence of particular qualities, or the degrees to which these qualities are present or absent, are frequently invented with this aim in view. For a large number of reasons it is difficult to devise suitable tests. It is, for example, extremely difficult properly to analyse the different qualities required in the performance of a given operation; it is difficult to know what differences there will be between abstracted specific qualities and qualities which are combined with other qualities in a test; it is difficult to be sure that a given test is truly analogous to the operation in question; and it is difficult to know that a test will be a true sample of the work which it is designed to test, when it is given in conditions which are artificially made to resemble natural conditions.

When these difficulties have been satisfactorily settled, and conveniently simple tests have been devised, there remains another difficulty of equal importance. Subjects very often perform the same test in different ways, so that a uniform system of marking is not appropriate. It is the object of this paper to show the large range of individual variation which can occur in the performance of a comparatively simple test, and to urge the desirability of combining subjective estimates with objective methods of marking.

II. DESCRIPTION OF EXPERIMENT.

The experiment was the ordinary Aiming Test with targets arranged as shown in the diagram below:



Target form reduced in size by one-half
Fig. 1.

Figures have been inserted under the targets in the diagram to simplify the discussion which follows; there were no figures under the targets on the form used in the experiment. The 128 subjects were army recruits of less than three months' service. The subjects held a metal pointer in their hand and kept time to a metronome set at 112. They tried to spear the centre of each target in turn, following the targets round in the order indicated by the numbers in the diagram until they reached the end, when they began over again. The subjects did not go direct from target to target but at every alternate beat of the metronome they returned to the starting position at *A*. Thus on the first beat of the metronome they speared target 1, on the second they returned to *A*, on the third they speared target 2, on the fourth they returned to *A*, and so on. Although there were no figures under the targets it was pointed out to the subjects that there was a line which connected each target with another, so that they could see quite easily which was the next target they were to spear.

The preliminary instructions were almost identical, and the experimenter performed the test himself in front of the subject. The subject then practised it and the experimenter corrected any mistakes he made. When the subject had performed two rounds in succession without making any mistake it was assumed that he had learnt the test. An unused target form was then set in front of him and he continued the test until told to stop. At the end of five rounds the order to stop was given.

It is usual to mark the test by measuring the distance of each prick made from the centre of its respective target.

It might be thought that a fair proportion of the subjects would readily understand the instructions so as to perform the two practice rounds and the five test rounds straight away without making any mistake, except in the accuracy of their aiming. It might also be thought that almost all, though perhaps making mistakes in the practice rounds, would eventually perform the actual test in the correct manner. However, 35 subjects (27·3 per cent.) performed the practice rounds as well as the test itself correctly; 35 subjects (27·3 per cent.) performed the test correctly, though they made mistakes in the practice rounds; the rest, amounting to 58 subjects (45·3 per cent.), made mistakes in the test after they had apparently understood how to do it.

The following is a list of the mistakes that were made in each round. The numbers indicate the order in which the targets were speared. It is advisable to refer constantly to the diagram of the target form in order to appreciate properly the significance of each mistake that was made.

III. DISCUSSION OF THE MISTAKES.

Table I represents only the order in which the targets were speared in each round separately. A further analysis was made of the mistakes made by each subject in the five rounds taken together. A part of this analysis is as follows:

In 19 cases, *i.e.* in the records of 19 subjects, the **765** mistake was made by itself, without being combined with any other mistake in any other round. In 9 of these cases the **765** mistake was made in only one round, in 7 cases it was made in two rounds, in 1 case it was made in three rounds and in 2 cases it was made in all five rounds. In 9 cases the **765** mistake was combined with other mistakes, such as the **7651098** mistake, in other rounds.

In 1 case the **1098** mistake was made by itself. In 3 cases the **1098** mistake was combined with other mistakes in other rounds.

Table I. Analysis of mistakes.

	Numbers making mistake in each round				
Mistakes	Round 1	Round 2	Round 3	Round 4	Round 5
12347658910	16	11	8	7	9
12347651098	2	5	2	1	4
12345671098	4	2	2	1	1
12345678109	1	1	2	1	1
12347651059	1	.	.	.	.
1234765981010	.	.	.	1	.
12347659810	.	.	.	.	1
1234765108	.	.	.	1	.
123476548910	.	.	.	1	.
1234*678910	1	1	1	.	1
1234*78910	.	1	.	.	.
12345*78910	.	1	1	.	.
1234567*910	.	.	.	.	1
123456789*	.	.	.	.	2
1*347658910	1	.	.	.	.
1234*67	.	.	.	1	.
12347*58910	.	.	.	.	1
123410678910	1	.	.	1	.
123465678910	1	.	.	.	.
1234105678910	.	1	.	1	.
132345678910	.	.	1	.	.
123456788910	.	.	.	1	.
123475678910	.	.	.	.	1
12347678910	.	1	.	.	.
12398735	.	.	.	1	.
1234656*8910	.	.	.	.	1
12341056*8	.	.	.	.	1
12345681098	.	1	.	.	.
12341	.	.	1	.	.
12345697810	.	.	.	.	1
91056789	.	1	.	.	.
112223344765889910	.	.	.	.	1
98765104321	1	.	.	.	.
12341098	1	1	1	1	.
1234108910	.	.	1	.	.
12341098789	1	.	.	.	.
12345109	.	.	.	.	1
34127651098	.	1	.	.	.
3541010987654321	.	.	1	.	.
12341095687	.	.	.	1	.
89114766655888991010	.	.	.	1	.
12345109687	1	.	.	.	.
123465108910	1	.	.	.	.
12354109687	.	1	.	.	.
469871682734109	.	.	1	.	.
1234567/8910	.	1	.	.	.
123456789/10/9	.	.	1	.	.

The last two mistakes were made by proceeding direct from one target to the next at the points marked '/.'

* Indicates that the intervening targets were omitted.

In 4 cases the **7651098** mistake was made by itself in one or more rounds.

In 5 cases the **7651098** mistake was combined with other mistakes in other rounds.

In 2 cases the **8109** mistake was made by itself.

The other mistakes for the most part appeared by themselves, but at other times they were combined with other non-recurrent mistakes, the extreme example being the record of one subject who, although he had performed the practice rounds twice in succession correctly, gave the following record: 1234**5109687** in the first round, 12**354109687** in the second, 12**354101098765432**1 in the third, 123**41098687** in the fourth, and 1234**5109** in the fifth.

Two important points about the mistakes that were made are:

(1) That in the vast majority of cases the errors occurred after the subjects had reached the end of a line, *i.e.* after they had speared either target 4 or target 7; and

(2) That the errors chiefly consisted in spearing all the targets *in a particular line* in the wrong direction.

It is suggested that these two facts indicate that the target form tended to be perceived by the subjects as composed of three Gestalten, each of which consisted of one particular line of targets. The mistakes that were made are more easily understandable on this assumption, which is not, however, elaborated any farther here.

The commonest mistake was the **765** mistake. Instead of spearing the targets in the correct order, the subjects in this group speared only the first four targets and the last three targets in the correct order, but reversed the correct order of the three middle targets. This mistake, like the other mistakes that were made, was not made consistently. It would appear in one round and disappear in the next. In a fair number of cases not counted as mistakes a tendency to commit this error could be observed. The subject would spear target 4 and then hesitate before proceeding to target 5. These hesitations are mentioned to show that the potentiality for making the mistake was greater than the figures actually indicate. The error itself is easily understandable. In writing we proceed in each line from left to right. In this test the position of the hands is the same as in writing and the right hand grasps a pointer that is similar to a pen or pencil. The threshold for the writing 'pattern' in the mind is therefore very low. Consequently, if the attention of the subject be relaxed during the experiment, part of the writing pattern may easily be called into operation. This, too, will explain the hesitation of some subjects at target 4.

The second mistake which was made, though not nearly so frequently, appears at first sight to contradict the explanation that has been offered for the **765** mistake. This was the **1098** mistake. In 9 cases it was combined with the **765** mistake in the same round, and in these cases it has been scheduled as *one* mistake, viz. **7651098.**

In the **1098** mistake the subject proceeded from right to left in the bottom line of targets instead of proceeding from left to right. An explanation of the mistake in the cases where it was combined with the **765** mistake might be that the subject remembered, while he was performing the second line of targets, that he ought to proceed from right to left in one of the lines, with the result that he proceeded from right to left in the third line. This suggested explanation lacks proof. Introspections were, unfortunately, not obtained from the subjects for the reason that the test was not used primarily for this study of individual differences.

The **8109** mistake was made by one subject in all five rounds. The explanation of this may be that the subject perceived the target form as composed of four Gestalten, target 9 which is on a lower plane than targets 8 and 10 being perceived as a distinct unit. This explanation is not entirely satisfactory because, for instance, target 2 is on a lower plane than target 3 and yet targets 1, 2, 3 and 4 were speared in the correct order.

One subject performed the **41098** mistake in the first four rounds and in the fifth round he speared the targets in the order 1234**10568910**, suddenly introducing the middle line of targets into his record.

One subject made the mistake 1234**10**678910, *i.e.* the substitution of target 10 for target 5, in rounds 1 and 4, but he performed the other rounds correctly.

All the other mistakes, including the omissions, substitutions and additions of targets, were made by the subjects in only one round of their test while they performed all the other rounds correctly.

IV. DISCUSSION OF METHODS OF MARKING.

The first question that arises is whether all the records together can be regarded as forming a homogeneous group. If the answer to this question is in the negative, what will be the best method of treating the records of the 58 subjects who made mistakes, so that they can be compared with those of the other subjects?

If it is required to obtain from the results a curve which will give the distribution of accuracy in the test, a curve from which can be drawn the mean, median and modal scores together with the standard deviation of the distribution, then in the first place the records of all the subjects can be included provided that all errors except simple accuracy of aiming tend to cancel one another out in the long run. In the second place, if all errors except simple accuracy do not tend to cancel one another out, all the records can still be included, provided that some reliable system of weighting the different mistakes so that they can be artificially made to cancel one another out can be evolved.

An examination of the errors made in this test shows that neither of these conditions is satisfactorily fulfilled. There are four groups of mistakes, viz. the **765**, the **7651098**, the **1098** and the **8109**, which tend to recur, but there are other mistakes which occur only once among all the records. There are two cases in which a subject added a target in one round and omitted a target in another round, but apart from these two cases there is no indication that the subjects who made one mistake in one round made a compensatory mistake in another. Even if there were it would be extremely questionable to assume that the omission of a target in one round was psychologically equivalent to the addition of a different target (or even the same target) in another round.

Furthermore, there is no satisfactory method of weighting the different mistakes. An inspection of the table shows that the non-recurrent mistakes cannot be regarded as of equal importance (for instance **112223344765889910** cannot be regarded as of equal importance to **468871682734109**), and there is no method of discovering their relative psychological importance. With regard to the recurrent mistakes the table shows that the four recurrent mistakes, viz. the **765**, the **7651098**, the **1098** and the **7651089** are made in the five rounds by the following numbers of subjects respectively:

Round 1	16	2	4	1
Round 2	11	5	2	1
Round 3	8	2	2	2
Round 4	7	1	1	1
Round 5	9	4	1	1

That is to say that the relative frequency with which they occur varies in each round.

The result is that no standard method of weighting can be evolved either for the recurrent or for the non-recurrent mistakes.

Consequently, either all the 58 records which show mistakes must be discarded or else the attempt to obtain an objective classification of the subjects as regards their accuracy of aiming must be abandoned. If the former position is adopted there are still further difficulties to be encountered. There are other individual differences to be taken into account. Such are the relative ability to keep time with a metronome correctly, differences in the amount of practice which each subject needs before he learns how to do the test, differences in the degree of hesitation shown by some subjects before or after they speared a target, and also the different targets at which this hesitation occurred. These are differences which it is difficult to measure correctly (in the case of hesitation it is almost impossible), but even if they are measured and weighted correctly yet it must be remembered that the introduction of artificial, objective measurements will mask interesting individual subjective differences.

V. CONCLUSION.

There remains the possibility of abandoning the attempt to obtain a purely objective measurement of accuracy in aiming. In this case the group method of applying the test must be discarded. Every record must be treated individually and a careful note made of the different ways in which each subject performs the test. The amount of practice that he needs before he has learnt the test; his persistence in making mistakes of different kinds in the practice rounds; his suggestibility to correction; his sense of rhythm; his natural speed of movement; his interest or boredom in the experiment; the nervousness with which he approaches it; his annoyance or indifference when he makes mistakes; all these qualities are observable if the subject is consistently watched. These observations, which should be combined where practicable with the subjects' introspections, are of great importance. They can be used with advantage to supplement each objective record before a final ranking of the different subjects for accuracy in aiming is decided upon. Vernon[1] has shown the need for making subjective estimates of temperamental qualities, and this paper attempts to show that tests which do not lend themselves completely to a system of objective marking are to be encouraged rather than discarded. They have a definite usefulness, and there is considerable scope for the further investigation of psychological characteristics along the line of a combination of subjective estimates with objective methods of marking.

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[1] P. E. Vernon, *Brit. J. Psychol.* 1929, xx, 97–117.

VI. SUMMARY.

1. The mistakes of 58 out of 128 subjects in performing five consecutive rounds of the Aiming Test are analysed (with the exception of individual differences in simple accuracy of aiming).

2. There are four groups of mistakes which tend to recur, though the relative frequency with which they occur differs from one round to the next.

3. There are many mistakes of varying degrees of complexity which are non-recurrent.

4. No satisfactory estimate of the relative importance of the different kinds of recurrent and non-recurrent mistakes can be obtained.

5. For a purely objective method of marking the subjects for accuracy in aiming, all the records of the 58 subjects who made mistakes must be discarded. This leads to a consideration of the rejection of other records for other reasons.

6. If the objective method of marking be supplemented by subjective estimates of various qualities possessed by the subjects, the records of all the subjects can be retained.

I wish to thank Dr H. Banister for the idea of writing this paper, and to Professor Bartlett I am indebted for valuable criticism.

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AN EMPIRICAL RESEARCH ON THE RELIABILITY OF CORRELATION COEFFICIENTS.

BY J. L. PRAK.

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II. *Description of method and tests used* (pp. 396–398).
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VI. *Conclusions* (pp. 401–403).

I. INTRODUCTION.

THE first attempt to throw light upon the reliability of correlation coefficients for small numbers of observations was made in an article published in *Biometrika*, VI, 1908–9, pp. 302–10, entitled "The Probable Error of a Correlation Coefficient." The author calls himself 'Student.' The material of which he made use was provided by measurements of (*a*) the stature, and (*b*) the length of the left middle finger of 3000 criminals. The correlation between these two was 0·66[1]. 'Student' divided his material into 750 groups, each of four persons, and by combining each group of four persons (i) with the tenth one before, and (ii) with the tenth one behind it, he obtained a further series of 750 groups, each of eight persons. He likewise determined the correlation coefficient for a series of 100 groups, each of thirty persons. These records he published in the form of a table.

'Student' then proceeded to compare the figures which he had thus obtained empirically with those which would be expected in accordance with mathematical formulae. He experimentally determined whether these formulae held for groups of four, eight and thirty cases, and he came to a negative conclusion for the first two cases, but to a positive one for the last one.

Applying to these conclusions the method I have used in my own study (which was necessarily on a much smaller scale), the results may

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[1] Correlations in this article are calculated according to the formula of Bravais-Pearson

$$r = \frac{\Sigma xy}{\sqrt{\Sigma x^2 \Sigma y^2}}.$$

be thus expressed. If, in groups of from four to thirty cases, the results were exactly the same as in one group of 3000 cases, one would expect in the cases of groups of four, eight or thirty persons a constant correlation 0·66 between stature and the length of the left middle finger. In point of fact this is no more true than if one claimed to be able to deduce the average stature of a population of 3000 persons from the stature of four, eight or thirty persons, or the school marks of a whole town from the school marks of one class.

In the probable error we have a measure for the expected deviations from the real correlation. As is well known, the probable error (which is 0·67449 of the standard variation) signifies something far more precise than its name might convey: it signifies the quartile of a normal frequency distribution; in other words, it means that there is as much chance that a value lies within the limits of the probable error as outside them. So, if we find a correlation 0·50 ± 0·04, this means that with a great number of correlation-calculations for equal groups, 50 per cent. of the correlation coefficients will be between 0·46 and 0·54. Thus the probable error gives a measure for the spreading of the correlation coefficients. If the formula $\text{P.E.} = \pm 0{\cdot}6745 \frac{1-r^2}{\sqrt{n}}$ still also holds for thirty, eight or four cases, then every time 50 per cent. of the correlations should be 0·66 ± P.E. But 'Student's' examination, described above, showed that with groups of four, eight and thirty cases 39, 43 and 58 per cent. respectively were within the limits. So in groups of four and eight cases the variation was greater than would have been expected according to the formula, and was smaller with groups of thirty. In other words, with such small groups as those of four and eight, one must reckon with a greater probable error than is given by the above-mentioned formula; that is to say, in the comparison between stature and length of the left middle finger; with groups of four and eight cases the probable error needs to be about 10 per cent. greater to contain 50 per cent. of the correlations, while in groups of thirty cases it is about 10 per cent. less.

However instructive this study may be, for our special object it falls short in two important respects. In the first place, the examination deals with physical phenomena; and although it is very probable that mental phenomena show similar variations and follow similar laws, we are not entitled without further inquiry to build upon such an analogy. For widely as the coefficients of variation for physical phenomena vary, those for the results of psychological tests fluctuate still more. Further, in a psychological research we have to reckon with errors of observation of a kind entirely different from those which occur in the afore-mentioned measurements. The results of psychological measurements depend on the clearness of instruction, the understanding of the subject, the degree of training, concentration of attention, competition, accidental interruptions, illumination, etc. Therefore in considering the results of a psychological test, we have to consider not only the errors of random sampling, but also the deviations inseparable from the very nature of the research.

In the second place, the number of groups employed by 'Student' suddenly leaps from four and eight to thirty: he neglects sizes which are in quite common use in applied psychology (ten to twenty). If we have recourse to interpolation in order to bridge this gap, we desert the empirical method.

II. DESCRIPTION OF METHOD AND TESTS USED.

In order to solve these difficulties, the following method was adopted. The correlation was determined between a number of tests and the total scores of a series of other tests, for an entire group of 152 subjects as well as for groups of thirty, twenty, fifteen, ten and six subjects. In this way, it was possible to examine empirically how far such small groups still gave a reliable correlation coefficient. As, besides the errors of random sampling, those deviations which have their cause in the nature of the examination are important, a short description of the tests and of the attending circumstances is here desirable.

The six tests, which were chosen for this research, were the following: correction of words, formation of words, analogies, intelligent memory, continuation of patterns and visual memory. The second series of tests, the *total* scores of which were correlated with the score of each test in this first series, consisted of another twelve tests of theoretical and practical intelligence, together with some occupational tests. All the tests in the two series were given in the entrance examination for the Eindhoven Trade School. (They are more closely described by me in *Mededeelingen van de D. Bos Stichting*, No. 13, Het Psychotechnisch Onderzoek aan de Ambachtsschool te Eindhoven, J. B. Wolters, Groningen, 1926.) To the results of these tests, with which the six above-mentioned tests were compared, marks were given according to importance (0–4, 0–8, 0–12, 0–16). By adding up the separate marks for each subject, a total result was obtained, which gave one a clear enough notion of the candidate's general suitability for a technical trade. The following is a detailed description of the six selected tests.

Correction of words. The examiner wrote the word 'dream' on the blackboard (*droom* in Dutch). Underneath he wrote words, formed out of the letters of 'dream,' like: 'am,' 'me,' 'dam,' 'are,' 'red,' etc. (*moor, oom, rood, moord,* etc. Dutch). Finally he wrote: 'dead' and 'rod' (*dam* and *dood* in Dutch). The boys were asked whether the words 'dead' and 'rod' could be formed out of the letters of 'dream.' When this question was answered in the negative, the two words in question were demonstratively crossed out, while the examiner asked why this was not possible. Thus, before starting, stress was laid on the character of probable mistakes, using a wrong letter, using the same letter twice, etc. Then the paper, which had been laid face downward on the table, was allowed to be turned. On this paper were two columns of words. At the top of the first column was the word 'spring-green.' The following instructions were given. "Now look at each word printed underneath, and see whether it can be formed out of the letters of 'spring-green.' If it can, leave it as it stands. But if it cannot, cross out the word, just as I did the words 'dead' and 'rod' under the word 'dream.' Ready—go"! Eight minutes were allowed for considering 120 words. When a number of boys drew near to the second column, the examiner said that the words in this one should be treated in the same way, but having regard to the new word standing at its head. The average result was 78·75 words; maximum 120, minimum 37, with a standard deviation of 18. For brevity's sake, I shall henceforth denote these by av., max., min. and σ.

Formation of words. Of three words, for which respectively two, three and three minutes were given, the following total numbers of words were formed: av. = 22, max. = 47, min. = 0, $\sigma = 6{\cdot}97$.

Analogies. Fifty analogies were to be sought: The numbers of correct completions were: av. = 15·13, max. = 39, min. = 0, $\sigma = 7{\cdot}83$. The numbers of wrong completions were: av. = 4·79, max. = 21, min. = 0, $\sigma = 4{\cdot}03$.

Intelligent memory. After a detailed example, ten groups, each composed of three coherent words, were slowly spoken. About half-an-hour afterwards this was repeated with another ten groups. The examiner gave the first word of each group in the original order, and the subjects had to write down the whole row of three words. With forty correct reproductions as maximum, we got the following results: av. = 20·62, max. = 38, min. = 0, $\sigma = 9{\cdot}65$.

Continuation of patterns. A number of five simple patterns, resembling plain border-decorations, had to be continued. Marks for each pattern were given from 1 to 5. The results were: av. = 12·26, max. = 21, min. = 5, $\sigma = 3{\cdot}43$.

Visual memory. Nine pictures were shown one by one for 8 seconds. The subject's reproductions of these were scored according to a scale of 5 marks. The absolute maximum was 45 marks, the results were: av. = 24·92, max. = 41, min. = 11, σ = 6·25.

III. EXPERIMENTAL PROCEDURE.

My 152 subjects were boys of 12, 13 and 14 years old. Seventeen boys had reached the fifth class of the grammar school, ninety-five boys the sixth, thirty-seven boys the seventh class, and three boys the eighth class.

The examination took place in groups of about thirty (twenty-eight to thirty-six), after an entrance examination in grammar and arithmetic held some days before. The duration of the examination was about five hours—from 8.30 to 11 o'clock in the morning in an ordinary classroom, and from 1.30 to 4 o'clock in the afternoon in a large arts' room, with short intervals in the two work periods.

I began with a questionnaire which had to be filled in. The selected tests were distributed over the whole day, forming respectively the sixth, first, fifth, thirteenth and seventeenth, eighth and nineteenth paper. The examiner was the same throughout the examination; so was his assistant. The examination was carried out under conditions as far as possible identical for each group, with the same examples and instructions, similar encouragement, etc. Some days before the examination a rehearsal was carried out on a small group of five boys (who did not take part in the examination) in order to make sure of a satisfactory standardization of conditions. Despite these precautions the examination did not prove to be so uniform as a series of tests daily repeated over a long period, or as the army intelligence tests where the instructions are mostly written on paper.

IV. FACTORS INFLUENCING THE RELIABILITY OF THE RESULTS.

The factors which influenced the reliability of the results may be divided into disadvantageous and advantageous ones. Among the latter, I may mention the homogeneity of the subjects, as regards their age, the constancy of time, testing-rooms, examiner, and of the incentive to effort (*i.e.* keenness to be admitted to the school, only half the number of applicants could be accepted). Among the disadvantages may be cited the comparative newness of the series of tests; the considerable differences in school training of the boys, which was not so much due to the number of different classes as to the great difference in quality of education (for further details see the above-mentioned pamphlet); the insufficient suitability of the class-rooms for a psycho-technical examination, compared with a room, specially equipped for it; the differences in illumination (sunshine, cloudy sky, distance from the window) and in distance from the examiner and from the blackboard. As a matter of fact, the boys with defective vision were given a place in the front row. Copying from each other was absolutely prevented in the afternoon and fairly well prevented in the morning session.

All things considered, the circumstances were neither more nor less favourable than they would be in a normal test examination in industrial psychology, and we can very well accept our results in this case as being normal, although they were not obtained under optimal conditions.

V. DISCUSSION OF EXPERIMENTAL RESULTS.

The correlations of the six separate tests with the criterion (total scores) upon which the tests have no influence themselves, were + 0·187, 0·440, 0·515, 0·528, 0·628 and 0·659 respectively. I expressly chose tests which correlated in a very divergent way with the criterion, in order to find for low and average, as well as for high, correlations the reliability of correlations with small groups. For 152 subjects the probable errors of these coefficients were ± 0·054, 0·044, 0·04, 0·039, 0·033 and 0·031 respectively. In order to obtain the smaller groups, the total scores and the results of the six tests for each of the 152 subjects were written on a slip of paper, and out of this material six groups of thirty, twenty, fifteen, ten and six were drawn. For the six groups of 30, a second drawing out of the total number was necessary, the others could be gathered at once. The correlations of these thirty groups were as follows: the probable error $\left(\text{according to the formula P.E.} = \pm 0{\cdot}6745 \frac{1 - r^2}{\sqrt{N}}\right)$ for the correlation coefficients of the total group are placed beside them, together with the average of the six correlations.

If the correlations of the small groups had been in exact agreement with the formula, so that for every test and in every group-number 50 per cent. of the coefficients fell within the limits $r \pm$ the probable error, then in the above table we should expect a 3 everywhere, and there should be 15 everywhere in the right-hand column and 18 in the bottom row. And the total number would then be 90.

It is evident that for every test and in every group-number the actual agrees fairly well with the theoretical figure. There are, of course, fluctuations with such small numbers, but the total result of ninety-six instead of ninety inspires confidence. The one considerable deviation

Table I.

Six groups of thirty persons.

Tests							Av.	P.E. ±
Correction of words...	0·210	0·128	0·231	0·236	−0·157	0·475	0·187	0·118
Derivation of words	0·602	0·523	0·227	0·451	0·449	0·579	0·471	0·099
Analogy	0·578	0·543	0·462	0·492	0·443	0·400	0·486	0·090
Intelligent memory	0·587	0·518	0·374	0·563	0·512	0·535	0·514	0·088
Continuation of patterns	0·645	0·587	0·575	0·691	0·712	0·695	0·650	0·074
Visual memory ...	0·709	0·732	0·698	0·590	0·496	0·645	0·645	0·069

Six groups of twenty persons.

Correction of words...	0·023	0·550	0·052	−0·192	0·314	0·202	0·159	0·145
Derivation of words	0·506	0·583	0·380	0·327	0·199	0·409	0 400	0·121
Analogy	0·578	0·446	0·514	0·259	0·302	0·669	0·461	0·110
Intelligent memory...	0·593	0·322	0·500	0·592	0·460	0·605	0·512	0·108
Continuation of patterns	0·750	0·417	0·747	0·498	0·885	0·431	0·621	0·091
Visual memory ...	0·809	0·548	0·555	0·671	0·698	0·588	0·644	0·085

Six groups of fifteen persons.

Correction of words...	0·241	0·681	−0·186	0·039	0·046	−0·188	0·092	0·168
Derivation of words	0·512	0·620	0·797	0·599	−0·289	0·476	0·452	0·140
Analogy	0·625	0·716	0·746	0·701	0·320	0·193	0·550	0·128
Intelligent memory...	0 830	0·579	0·600	0·471	0·475	0·466	0·536	0·125
Continuation of patterns	0·714	0·663	0 845	0·834	0·731	0·398	0·697	0·105
Visual memory ...	0·580	0·578	0·612	0·788	0·633	0·545	0·622	0·098

Six groups of ten persons.

Correction of words...	−0·179	0·207	0·712	0·293	−0·430	0·060	0·110	0·205
Derivation of words	0·372	−0 015	0·638	0·841	0·587	0·116	0·423	0·172
Analogy	0·914	0·476	0·808	0·468	0·579	0·619	0·644	0·156
Intelligent memory...	0·596	0·763	0·840	0·537	0·733	0·423	0·648	0·153
Continuation of patterns	0·652	0·137	0·607	0·334	0·391	0·344	0·410	0·126
Visual memory ...	0·226	0·546	0·794	0·817	0·532	0·545	0·576	0·120

Six groups of six persons.

Correction of words...	−0 114	0·594	−0·271	0·759	0·404	0·194	0·261	0 266
Derivation of words	0·557	−0·247	−0·344	0·666	0·844	0·250	0·287	0·222
Analogy	0·442	0·310	0·086	0·621	0·645	0·352	0·380	0·203
Intelligent memory...	0·432	0·507	0·229	0·675	0·377	0·951	0·528	0·199
Continuation of patterns	0·870	0·945	0·725	0·639	0·386	0·909	0·829	0·167
Visual memory ...	0·207	0·764	0·740	0·506	0·762	0·721	0·616	0·156

Coefficients falling within r ± P.E.

Number in group ...	30	20	15	10	6	Total
Correction of words ...	4	3	2	3	2	14
Derivation of words ...	3	4	2	2	3	14
Analogy	5	3	1	4	4	17
Intelligent memory ...	5	5	4	3	4	21
Continuation of patterns	5	0	3	2	2	12
Visual memory ...	4	3	4	2	5	18
Total	26	18	16	16	20	96

(twenty-six) occuring in the group of thirty, is on the safe side. As regards the situations of the largest deviations, those which amount to two, three or four times the probable error, practice and theory agree again rather well.

With an unlimited number of correlation coefficients, it may be expected that one in six deviations will be greater than twice the probable errors, that one in twenty-three will be greater than three times, and one in 143 will be greater than four times the probable error. In regard to our 180 correlation coefficients, theory and practice conform as follows:

Table II.

Deviations		Correlations of						
Greater than	According to the formula	30	20	15	10	6	Total	According to formula
1 × P.E.	18	10	18	20	20	16	84	90
2 × P.E.	6	4	7	9	9	6	35	30
3 × P.E.	1·6	0	0	1	3	2	6	8
4 × P.E.	0·25	0	0	1	0	0	1	1·26
5 × P.E.	0 029	0	0	1	0	0	1	0·144

As regards the larger deviations, too, the coefficients fall with sufficient accuracy within the limits denoted by the formula.

VI. CONCLUSIONS.

We may conclude then, so far as the limited extent of this examination justifies the conclusion, that the formula for the probable error in our groups actually holds, although the groups of thirty and twenty persons give a more favourable result than those of fifteen, ten and six.

The number of deviations which is greater than twice the probable error is 50 per cent. more in the groups of ten and fifteen than would be expected according to the formula. Nine however of these eighteen large deviations are caused by the two lowest correlating tests, while three tests have only four of these large deviations and give therefore a better result than might have been expected.

Another question is: When is the correlation calculation still practically reliable? The deviations with ten and six persons approximately correspond with the formula, but the formula may also admit practically useless coefficients.

In reality, numbers which can vary 0·40 and more, are of little importance. We see, however, that the average of six groups of six approaches the real coefficient to less than 0·10 in three of the six tests, while only one average correlation deviates 0·20 from that of the total group. In the six groups of ten, the maximum deviation of the average correlation is 0·218, but five deviations remain below 0·14 and three below 0·085. In the six groups of fifteen, the maximum deviation of the average correlation is 0·095 and the others remain below 0·07, while four deviate less than 0·04. In the groups of twenty the greatest deviation is 0·054 and three are smaller than 0·02. So we find, as 'Student' found, that a correlation from groups of thirty subjects is reliable, although repetition will strengthen this reliability. Roughly speaking, three coefficients out of four are fairly correct (their deviation does not exceed 0·07). In groups of twenty persons the reliability already diminishes, though it still corresponds perfectly to the formula for the probable error. Repetition of the examination with other groups of twenty is, therefore, still more desirable. With two or three groups the reliability is already fairly high; that is to say, reliability does not mean here that the result is absolutely correct and that it will not alter when the examination is extended, but a certainty which allows to judge whether a test for a special pedagogical or industrial purpose is useful or not and which makes it possible to determine roughly the total value of a series of tests for such a purpose. Groups of fifteen already show rather large deviations; only thirteen of the thirty-six coefficients deviate less than 0·10 from the real value and the number which deviates more than 0·20 is also thirteen. It is necessary to examine three or four groups of fifteen in order to get a reliable result. With groups of ten one should have at least six groups, and more than six with groups of six.

The average deviation of the thirty-six coefficients for each group from the 'real' value (with 152 boys) is for thirty, twenty, fifteen, ten and six respectively 0·077, 0·120, 0·176, 0·209 and 0·248. For the two tests with the greatest deviation (*i.e.* the lowest-correlating tests) these numbers are about 40 per cent. higher; while for the two tests with the smallest deviation (intelligent and visual memory) they are 31 per cent. lower.

Generally a number of at least fifty subjects is desirable before being able to speak with certainty. This does not exclude the fact that a careful examination of a small number of subjects followed by correlation calculation is the most reliable method and is to be preferred to subjective estimation of the relation between the variation series as is occasionally done by vocational psychologists. But such a limited material is insufficient to draw wide conclusions and the examiner will do well to expect large variations when eventually he repeats his examination and so make allowances for these in the practical steps which he may adopt.

In such a case it is preferable to spend money, time and energy on repetition of the examination rather than on more subtle calculations and methods of correlation. These latter do lead to higher correlations, it is true, but they do not always rest on an equally firm basis.

Another question is whether it is not possible to get the same results with a small number by appropriate selection from a larger group; *e.g.* by selecting, as regards the variation-series, a representative group. In the present instance those groups were selected which were uniformly constituted as regards the total score in the 12 tests; that is in regard to the criterion. Thus we selected two groups of thirty and twenty, four groups of fifteen and ten, and six groups of six persons, which were uniformly spread over the scale of the total score and we again calculated their correlation with the six tests. Results were not materially altered, neither for better, nor for worse.

Finally I have to offer my best thanks to Dr C. S. Myers for all the trouble he took in kindly correcting and polishing my rather bad English.

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THE COMPARATIVE PSYCHOLOGY OF JEWS AND NON-JEWS: A SURVEY OF THE LITERATURE.

BY J. RUMYANECK.

(*Under the auspices of the Jewish Health Organization of Great Britain.*)

IN view of the paucity of exact knowledge as regards the psychological differences between races, it is surprising to find each race endowed by anthropologists and sociologists with attributes peculiar to itself, and which are hereditarily determined. Thus, according to Crew in his *Inheritance in Man* the Nordic race is "prudent, self-controlled, its sensuous instincts subordinated to higher aims, courageous, sea-faring, athletic." The Mediterranean peoples, on the other hand, "are vivid, volatile, easy in self-expression, take life not too seriously and are not renowned for adherence to truth or loyalty." The Negro is "improvident, boisterous, or else dejected, lacking creative power, imagination and ambition, is cruel."

In the following essay an attempt is made to collate the judgments that have been passed on the Psyche of the Jew and especially on his intelligence, which is supposed to be superior to that of other peoples. The results of a number of intelligence and emotional tests that have been applied are critically examined, and the view is expressed that the claims of intelligence testers as being able to reveal innate psychological differences among different groups have in no way been substantiated; that although the Jews may be intellectually superior and may possess unique psychological faculties, no technique is in existence at present for their evaluation. The former trend in thought which ascribed to heredity or nature preponderant weight in the formation of these psychological differences is found to be untenable, and the view is suggested that in dealing with large groups of people as distinct from special family lines, environment is of overwhelming and pervasive importance.

Qualified observers in England of the Jewish community have always been struck by the intelligence of the Jewish children. C. Russell (*The Jew in London*, 1900) writes: "The foreign children in the East End are universally allowed to be sharper and more intelligent than the English, and they carry off a large proportion of prizes and scholarships." "I was astonished at their intelligence," said Sir C. Cameron before the

Inter-Departmental Committee on Physical Deterioration in 1904, and Colonel Fox thought that the Polish, Russian and Roumanian Jewish children were of higher intelligence than the English children. Two years previously witnesses before the Royal Commission on Alien Immigration expressed similar opinions. Mr G. L. Bruce, a member of the School Board in charge of the Whitechapel schools said: "The Jewish children have proved excellent scholars." Messrs J. M. Myers, S. Mather, F. H. Butcher and W. A. Nugent all thought that intellectually the Jewish child is superior to the non-Jewish child.

In 1916 in Germany O. Nemeck (1) analysed the scholastic records of 1549 pupils of a commercial school and found that the Jewish children's attainments were of a higher standard in languages, mathematics, chemistry, physics, shorthand and mathematical geometry. In natural history, geography, drawing, punctuality, diligence and behaviour they were also superior. The author believes in "eine unleugbar stärkere intellectuelle Veranlagung der jüdischen Schüler," but leaves open the question whether this superiority is due to earlier maturity among them or to an innate superior function. Contrary results were found by Jacobs (2) in 1922 who, examining the reports of 10-year-old children in reading, writing, spelling, arithmetic, geography and history, concluded that the Jewish boys are on a slightly lower level and are more variable in their attainments than non-Jewish children.

In New York in 1920 Miss Murdoch (3) endeavoured to throw light on this problem by the application of intelligence tests. She tested 489 Jewish children, 485 American and 491 Italian children, chiefly of the ages 10, 11, 12 and 13. The Jewish children were found to be on approximately the same level as the American children, and far superior to the Italians and the Negroes, the latter doing better in the tests than the Italian children. In the three largest age groups, 10, 11 and 12, the average was 15·5 per cent. for Italians, 30·5 per cent. for the Negroes, and 53·7 per cent. for the Americans, equalizing or exceeding the median for the Jewish children. The tests, however, involved the use of verbal concepts and introduced a language handicap chiefly felt, it seems, by the Italian children, and which makes the appraisal of the results difficult.

Pintner and Keller (4), in 1922, using a revision of the Binet test, examined school children in Youngstown, Ohio. The average I.Q. for the seventy-nine Jewish children, irrespective of whether they spoke English or a foreign language, was 95. The following table gives the scores of the English- and foreign-speaking groups:

English-speaking groups			Foreign-speaking groups		
No. of cases	Nationality	Average I.Q.	No. of cases	Nationality	Average I.Q.
249	American whites	95	313	Italian	84
71	American coloured	88	130	Slavic	85
24	English	97	37	German	91
			18	Roumanian	97
			4	Polish	85
			10	Russian	89

The I.Q. for the total English-speaking groups is higher than that for the foreign-speaking groups.

	No. of cases	Average I.Q.	Median I.Q.
English-speaking	367	92	94
Foreign-speaking	674	84	85

In Performance tests, however, the scores of the foreign-speaking children were higher. The author writes: "In comparing results obtained from a group of cases, given the Stanford Revision of the Binet test and a series of Performance tests, we find the correlation between the tests considerably better for the English-speaking group than for the foreign-speaking group and there were 23 per cent. more cases of the foreign-speaking children than of English-speaking where the performance age was higher than the mental age." It can hardly be doubted that the language handicap partly accounts for the lower scores of the foreign-speaking groups.

It may be interesting in this connection to glance at Brigham's (5) analysis of the intelligence of American recruits during the War, which was published in 1923. Surprisingly he finds the intelligence of the Jewish adults to be below that of all other nationalities except Polish and Italian. He also observed great variability amongst them, which fact he thinks would explain the popular belief in the superior intelligence of the Jews. "An able Jew is popularly recognized not only because of his ability but because he is able and a Jew." Brigham, however, makes no allowance for the fact that many of the soldiers could not speak English well; and, when he finds that those who had been a longer time in the country do better in the tests than the more recently arrived immigrants, he adopts the fantastic assumption that the recent immigrants are innately of a lower intelligence. To explain the higher scores of the northern Negroes as compared with the southern Negroes, he assumes that, by a selective process, the more energetic and able Negroes emigrate to the north. Rather than accept this assumption for which no evidence exists, it seems more probable to suppose that the environmental *milieu* of the northern Negro is more conducive to intellectual development than the environment of the south, and that the effect of this superior environment is reflected in the intelligence scores.

Feingold (6) in 1924 in a careful and detailed study found only slight differences between the American and foreign-born whites. Applying a modified form of the Army Alpha to 2353 high-school freshmen he concluded that:

(1) The mental differences between foreign children reared in an American environment and children of Anglo-Saxons are too small to be significant.

(2) Jewish children of high-school age have almost the same mental rank as American children in their freshman year, and are only a few points lower in their junior year.

(3) Increasing differences in social opportunities after the sixteenth year account for the increasing differences in the scores between the high-ranking and low-ranking racial groups.

In this study the language handicap did not exist as all the high-school freshmen would speak English well. The Jewish pupil's slight inferiority in his junior year can be explained, as Feingold points out, by reason of the environmental check to his development as he grows older. He meets with a more restricted field of development than does the child of Anglo-Saxon parents. Feingold also observed that, whatever the social standing of the Jewish child's parents, every endeavour is made to send the child to a high school and give him encouraging stimuli to do well in school. It is impossible to overrate the importance of these factors in any study of the intelligence of the Jewish child.

Another important factor in this connection is the supposed earlier maturity of the Jewish child: W. F. Dearborn (7) stresses this point. In an investigation which he conducted in 1924 he examined almost equal number of Jewish, Italian and American children living in the same district. The Jewish children ranked highest both in mental age and intelligence quotients. They also, however, ranked first in anatomical development, as indicated by the stage of ossification of the carpal bones and expressed in terms of anatomical ratios. He maintains that: "The Jewish superiority and intelligence is partly due to greater anatomical and probably correspondingly greater physiological development, in that the slower growing American group will either develop for a longer period, or that their growth will be more accentuated during its later stages. So that at the completion of the period of growth, the critical disparity between the intelligence of the two groups would disappear. This result would depend upon the duration of the period of growth and the amount of later acceleration. If both factors were operative the American group may in the end excel in intelligence." Much further research, however, is needed before these points can be determined.

A. Fukuda (8) in 1925 found the median I.Q. of Jewish children to be 82, which was much lower than the score of the English-speaking groups and lower than the scores of the foreign-speaking groups except Italian children. He had, moreover, only three Jewish children in his sample. The following are the scores of the English- and foreign-speaking groups.

	No. of cases	Median I.Q.	Average I.Q.
Total English speaking	109	96	95
Total non-English speaking	148	92	91

V. Graham (9) in a study of children at habit clinics applied the Stanford test and found that the Jewish child came out best. Below are given the scores of the different groups:

Nationality	No.	Median I.Q.
Jewish	47	105
American	76	99
Italian	43	85
All nationalities and others	167	95

In this investigation the least variability was exhibited by the Italian children.

In 1925 Karl Pearson (10) published the results of a survey which he commenced in 1913 into the problem of alien immigration into England. His results show that not only physically but mentally the Jewish children were inferior to the non-Jewish children. He analysed teachers' estimates respecting 619 Jewish boys and 585 Jewish girls, and found that "Jewish girls have less intelligence than the Gentile girls in any type of Council School. The comparison of Gentile and Jewish boys is less clear cut.... They (the Jews) are not as good as the boys of the medium or average school, but are better than the boys of the poor type of school. What is definitely clear, however, is that our alien Jewish boys do not form from the standpoint of intelligence a group markedly superior to the natives. But that is the sole condition under which we are prepared to admit that immigration should be allowed." How far this desire, so abruptly introduced in the last sentence quoted, coloured the investigation it is difficult to say. A later investigation carried on by Davies and Hughes (16), who applied standardized intelligence tests, arrived at conclusions completely opposed to those of Pearson. The following tables are taken from Pearson's survey:

Frequency distribution of intelligence—alien Jews.

	Very able	Capable	Intelligent	Slow	Dull	Very dull	Mentally defective
Jewish boys (%)	3·1	12·9	36·2	34·7	11·0	1·9	0·2
Jewish girls (%)	1·2	6·3	25·5	37·8	18·1	10·4	0·7

Intelligence distribution in twelve London schools of native Gentiles.

	Particularly clever	Above average	Average	Below average	Very dull
Gentile boys (%).	10	25	45	17	3
Gentile girls (%)	8	19	53	16	4

In criticism of Pearson's results it may be pointed out:

(*a*) Pearson's data as regards the intelligence of Jewish children consisted of teachers' estimates, which are very unreliable.

(*b*) The ratings for the Jewish and Gentile children were made by different teachers.

(*c*) Nearly one-third of the children (28·3 per cent.) were born abroad and not all knew the English language well. Only 6 per cent. of the fathers were born in England or naturalized, while 94 per cent. were foreign-born, and in these homes English would not be spoken.

(*d*) 71·9 per cent. of the fathers were engaged in occupations of low-grade labour. And although, as Pearson points out, the occupation of the father is no criterion of the innate intelligence of the child, it certainly influences its manifestation. A number of investigations have shown that the children of the economically more comfortable classes rank higher in intelligence tests than those who are lower in the social scale.

(*e*) The Jewish children surprisingly were found to contain a larger number badly nourished than the Gentile children. This must have had some effect on their school work.

(*f*) Pearson's main thesis that no correlation exists between intelligence on the one hand, and physique, health, care of the parents, economic and sanitary conditions of the home, on the other, is not yet conclusively established.

An American investigator, Miss Luckey (11), working on the same problem in 1925 in Cleveland, found that the Jewish children ranked only a few points below the American children and were second to them. The rank order of the groups was as follows:

(1) American children.
(2) Jewish ,,
(3) Hungarian ,,
(4) Bohemian ,,
(5) Coloured children.
(6) Polish ,,
(7) Italian ,,

In a second survey she examined 14,000 children who had been through a clinic, which generally received those who were above and below normal. The majority of the children were between 5 and 8 years though the range was from 4 to 21 years, and both sexes were lumped together. No national or racial group had less than 200 representatives; the Terman intelligence tests were used. Although the Jewish group had only as many as were examined in the Polish group, it had over forty times as many children who could be called bright: it had, on the other hand, less than a quarter as many children who were so seriously retarded that the regular school work could not be given them. The Jewish group had the largest percentage of bright children and the smallest percentage of retarded, as the following figures show:

Percentage of children classed as borderline or below (I.Q. below 80)	
Negroes	65
Polish	65
Slavish	63
Slovenian	58
Italian	55
Hungarian	47
German	46
Bohemian	41
American	30
Jewish	29

Percentage of bright children (I.Q. above 110)	
Jewish	24
American	22
German	8
Bohemian	6
Slovenian	3
Hungarian	2
Slavish	2
Polish	1
Negro	1
Italian	½

As regards children classed as 'Imbeciles,' the interesting fact emerged that the Jewish and American groups had as many as any other group, in fact, more than the Negro and Slovenian groups.

A preponderance of gifted Jewish children was also noticed by Terman (12). Although the proportion of Jews in the three main cities from which the gifted children were selected was 5 per cent., more than 10 per cent. of the children were of Jewish blood.

In 1926 Miss Bere (13) made a study of 10-year-old boys of foreign parentage in New York, by means of the Stanford-Binet, Pintner-Patterson performance, National, and Pintner non-language tests. On the Stanford-Binet, Pintner non-language and the National tests, the boys ranked:

(1) Jews. (2) Bohemians. (3) Italians.

On the Pintner-Patterson performance tests, however, they ranked:

(1) Bohemians. (2) Italians. (3) Jews.

A similar study by N. D. M. Hirsch (14) who tested 5504 children in schools from Grade I–IX inclusive, chiefly by the Pintner-Cunningham primary tests, revealed that the Jewish children scored best. The rank order according to the scores was as follows:

(1) Polish Jews.
(2) Swedes.
(3) English.
(4) Russian Jews.
(5) Germans.
(6) Americans.
(7) Lithuanians.
(8) Irish.
(9) British Canadians.
(10) Russians.
(11) Poles.
(12) Greeks.
(13) Italians.
(14) Negroes.
(15) Portuguese.

The author believes that the differences in intelligence are national or natio-racial, rather than social. It is difficult to see, if this is the case, why Polish Jews rank higher than the Russian Jews. Racially, both groups are very much alike; it is more than probable that environmental differences account for the superiority of one over the other. The author further believes that the language factor is negligible, and that intelligence is related to occupation as cause is to effect. Both of these assumptions have not the slightest evidence to support them.

Miss F. L. Goodenough (15) also believes that racial differences in intelligence have been established. She is of the opinion that the foreign groups are not handicapped by the language difficulty, since, in performance tests, equal ratings do not follow, ignoring the other and more pervasive influences affecting the result. She also maintains that an inferior environment is an effect, as much as it is a cause, of inferior ability, as indicated by intelligence tests. Dealing with large numbers of individuals as distinct from a defective line, which may sink into a low environment because of inferior intelligence, there is nothing to justify her beliefs, and even with such lines, taking the Jukes and Kallikaks, we do not know how many would be decent citizens had they been blessed with a better environment. After examining 2457 American-born young children in California, from various stocks, by the Goodenough tests, which consist chiefly of drawing exercises, and which eliminate any language difficulty, Miss Goodenough comes to the conclusion that superiority in intelligence lies with the Jewish children, whose median I.Q. was 106·3. She found that the coefficient of variability is least for the Jewish group indicating, she maintains, either the effects of selective immigration or ethnological purity. Brigham (5), on the other hand, as we have shown above, credits them with a high variability. The results she obtained are given in the following table.

The author claims validity for her results in that they correspond closely to the rank order of tests which involved the use of language. This, however, is no argument for accepting the results, since agreement between different tests proves nothing, especially as the tests used different concepts and were measuring rods with no fixed units for measurement.

Stock	No.	Median I.Q.
American	500	100·3
Armenian	123	91·8
Italian	456	87·5
Spanish Mexican	307	87·2
Californian Negroes	69	82·7
Southern Negroes	613	76·5
Hoopa Valley Indians ...	79	85·6
Jewish	55	106·3
Chinese	25	103·1
Japanese	42	99·5
Germans	29	98·8
Portuguese	11	93·9
English and Scotch	14	99·5
French and Swiss	14	97·8
Danes, Swedes, Norwegians ...	31	104·5
Assyrians, Slavonian, Serbians	29	94·5

One of the most thorough investigations into the intelligence of Jewish and non-Jewish children was that of Miss M. Davies and Mr A. G. Hughes (16) in 1927. Nearly 2000 children in London from 8 to 14 years of age were given the Northumberland standardized tests (1925 series) which consisted of exercises in general intelligence, problems in arithmetic, and English, reading, spelling, history, geography and composition. The results were checked by the ratings of teachers who were in complete ignorance of the test results. In order to eliminate differences in environment, three London schools were chosen, each representative of a different social *milieu*, and each possessing approximately equal numbers of Jewish and Gentile children. The following are the authors' conclusions.

> 1. On an average, both in general intelligence and in attainments in English and arithmetic, Jewish children are definitely superior to the non-Jewish children attending the same school, the superiority being more marked with the boys than with the girls. This superiority is found at every age from 8 to 13 and in every type of elementary school tested. The superiority in intelligence revealed by the tests is confirmed by the independent estimates of the teachers.
>
> 2. The racial difference in favour of Jewish children amounts in the case of boys to about one year in intelligence and in arithmetic, and to a year and a half in English at age 10. Among girls the differences are equivalent to a year in English and to slightly less than a year (0·9) in intelligence and arithmetic.
>
> 3. Variations in the differences between Jewish and non-Jewish children are found at different ages, the noticeable and consistent being a temporary drop at age 12 followed by a distinct rise at age 13. There is no indication in the data (which concerns children from 8 to 13 years of age) that the Jewish children's precocious development is followed by a premature arrest.
>
> 4. No sex difference in English and arithmetic is found among non-Jewish children: Jewish boys are on the average superior in these subjects to Jewish girls (the equivalent of about four months' progress at 10 years of age).
>
> In intelligence the Jewish boys are slightly superior to Jewish girls, but the contrary result is found among non-Jewish children.

However ably the investigation was carried out, it is doubtful whether the environment in the widest sense of the word was the same for both groups of children. "The choice of schools," the authors write, "was designed to rule out differences due to variations in school training and in home training (except in so far as the latter are due to racial customs and traditions)." In "racial customs" and traditions we already find a difference in environment. Elsewhere Mr Hughes (17) shows that the Jewish children were better fed and clothed, that the economic position of the Jews is less static than that of Gentiles and that, even in poor districts, they may live on a higher scale than their neighbours; finally that the Jewish child may develop more quickly than the Gentile child. Nevertheless the author believes that the occupational and social differences between the two groups was not great, and his belief in the Jewish child's superiority is strengthened by an analysis of the intelligence of the Jewish and non-Jewish children in different occupations. He classifies the occupations of the parents into the following groups:

(1) Manufacturers, merchants, travellers, Rabbis, teachers, higher grade clerks;
(2) Engineers, electricians, cabinet makers, metal workers;
(3) Shopkeepers, dealers and small masters;
(4) Policemen, postmen, bus and taxi-drivers, conductors, soldiers, sailors, railway porters;
(5) Makers of boots, clothes, umbrellas, buttons, boxes, cigarettes;
(6) Bricklayers, carpenters, painters, plumbers;
(7) Shop assistants, hairdressers;
(8) Carmen, coalmen, caretakers;
(9) Labourers (mainly in docks), hawkers;
(10) No father;
(11) No occupation;

and finds the differences between the children in similar occupational groups to be:

Group	1	2	3	4	5	6	7	8	9	10	11
Jews	109·7	107·7	110·1	105·4	103·9	105·7	102·0	112·4	103·8	100·6	85·8
Non-Jews	103·5	101·3	98·4	101·5	97·3	93·7	88·7	92·8	90·1	93·0	90·7
Difference	6·2	6·4	11·7	3·9	6·6	11·4	13·3	19·6	13·7	7·6	4·9

In Groups 4, 6, 8, 9 and 11 there were less than twenty children.

From an analysis of this table the author concludes that, in Groups 1, 2 and 5 the differences on the average was 6·5 or half a year at the age of 10, and that in Groups 3 and 7, since the differences were twice as great, the more brilliant Jewish children are indicated, commerce-attracting, so his argument runs, Jews of very good intelligence.

What, however, seems to be more significant, if we include Groups 4, 6, 8 and 11, is that the difference between the Jews and non-Jews increases as we go from the higher occupational groups to the lower. This is what we should expect if we compared the environment of Jewish and non-Jewish children whose fathers were merchants, and whose fathers were carmen. The difference in the latter case would be much greater, as could be testified by anyone who is acquainted with the social conditions of Jewish and non-Jewish working men. Whereas the difference in Group 1 is 6·2, in Group 8 it is 19·6. The home conditions of an English or Jewish coalman or caretaker would be worlds apart. The stimulus and encouragement to study and the sobriety of the wage-earner, which are found in the lowest of Jewish homes and which are a part of the Jewish tradition, are not found as frequently in similarly situated English homes. The assertion of the author that the best type of Jew goes into commerce is an unproved statement, and certainly he has not proved that "after environment is eliminated there still remains a racial difference equivalent at the age of 10 to about 0·7." Environment has not been eliminated, and it is this factor which probably accounts for the superiority of the Jewish children.

Miss Irma Loeb Cohen (18), in her thesis on the comparative intelligence of the Jews, also found a small but consistent superiority of the Jewish over the non-Jewish students at Ohio University.

Seago and Koldin (19) examined sixth grade pupils of Jewish and Italian nationality. Although no language handicap was involved the Jewish students scored higher than the Italians.

Prof. E. C. Hayes (20) reviewed the scholastic records of some racial groups in the University of Illinois and obtained the following rank order:

(1) Chinese.
(2) Jews.
(3) Nordics.
(4) Foreign students (excluding Chinese).
(5) Alpine.
(6) Mediterranean.
(7) Negroes.

The average record, however, of each racial group was below that of the general mixed group.

The language handicap was investigated by M. Rigg (21) in 1928. He tested Grades III–VIII in elementary schools, in the National intelligence tests which partly involves the use of language, in the Woody-McCall arithmetic test which hardly involves language, and the Thorndike-

McCall reading test which is essentially a language test. A glance through the subjoined table shows that a definite language handicap existed among the foreign groups:

		Intelligence quotients		Arithmetic quotients		Reading quotients	
Group	No.	Median	P.E.	Median	P.E.	Median	P.E.
Native	8130	104·85	0·17	102·96	0·15	100·28	0·17
Foreign	1949	103·30	0·34	104·26	0·31	98·06	0·35
German	1095	104·69	0·44	104·64	0·39	99·61	0·45
Jewish	445	103·19	0·82	106·10	0·72	98·35	0·76
Italian	140	91·43	1·13	93·81	1·28	86·74	1·11
Bohemian	118	104·00	1·22	104·56	1·28	96·67	1·39
Miscellaneous	151	105·14	1·08	104·33	1·04	98·47	1·18

In spite of a reliable difference in favour of the native group on the intelligence tests, there is a reliable difference in favour of the foreign group in the arithmetic test. In the reading test, which was largely a language test, the native group did best. Although every group did not score as well on the reading test as on the arithmetic test, the foreign children fell short to a much greater extent than the native, as an analysis of the drop between the median arithmetic quotient and the median reading quotient, for the various groups, shows:

Native	2·68	Italian	7·07
Foreign	6·20	Bohemian	7·89
German	5·03	Miscellaneous	5·86
Jewish	7·75		

According to the author the language handicap by itself is sufficient to explain the lower standing of most of the foreign groups on the intelligence tests. The German Jews and Bohemians, it is to be noticed, are on about the same level, whilst the low rank of the Italians cannot be accounted for, writes the author.

Several surveys have shown that the language handicap is of great importance in determining the results of intelligence tests. G. L. Brown (22) has shown that immigrant children rated 18 months' higher when tested in their own language. Mead (23), testing Italian children in America, found that the mean score in the tests is positively correlated with the amount of English spoken in their homes, with the period of time their fathers lived in America, and with their progress up the higher grades in the school. Walters (24) came to the conclusion that there is a language handicap in the Stanford Revision of the Binet-Simon tests of 6 to 8 months' mental age for foreign children of 13 years. Flores (25) studied English and Spanish children in the same school, and found that there were significant individual differences in the degree to which the language difficulty is experienced among the Spanish-speaking children. Everywhere children have ranked higher in tests not involving the use of verbal concepts than in those where such concepts were employed.

Of the nineteen investigations listed above, eight maintained that Jewish children were superior to non-Jewish children, whether in England, Germany or America, four that they were inferior, and four that no appreciable differences exist; in two of the investigations the Jewish children compared with Italians and Bohemians were found to be superior, and in one, where scholastic excellence was the criterion, the Chinese were first and the Jews second. In some instances the superiority or inferiority was of such a slight character that it becomes doubtful whether it is permissible to use such descriptive terms at all. Even if differences in intelligence do exist between the various national or racial groups, its expression, as measured by the tests, was so overlaid by so many other factors that it becomes almost certain that we are not dealing with any innate differences. And if this be the case there is no cause for checking the immigration into England or America on biological grounds of 'inferior' East European peoples. As regards the Jews the consensus of opinion that they are superior in intelligence may be correct, but this superiority has not been proved to be hereditary, and probably has its roots in the traditions, education and general superiority of the Jewish environment.

The claims that were made for intelligence, when they first spread through the educational world, have not been substantiated. The reaction to the view that they are an innate measure of ability is very pronounced to-day. For even if we are agreed upon a satisfactory definition of intelligence—and there is more agreement as to who is intelligent than what intelligence is—the impossibility of evaluating the importance of environmental factors, which alter the expression of intelligence, makes the tests almost worthless in value for ordinary school purposes. Their sphere of greatest utility lies, in the present at any rate, in the field of somewhat subnormal intelligence. As tests of retardation in intelligence and school development they are of great value. Intelligence tests have certainly not measured what intelligence is as defined in the definitions given below:

(1) Intelligence is judgment or common sense, initiative, the ability to adapt oneself. (Binet.)

(2) Intelligence in general is the power of good responses from the point of view of truth or fact. (Thorndike.)

(3) Voluntary attention is the essential factor of general intelligence. (Burt.)
(4) Intelligence is intellect plus knowledge. (Hemmon.)
(5) Intelligence is an acquiring capacity. (Woodrow.)
(6) Intelligence is the ability to think in terms of abstract ideas. (Terman.)
(7) Intelligence is the relative general efficiency of minds measured under similar conditions of knowledge, interest and habituation. (Ballard.)

Nor have the tests been applied "under similar conditions of knowledge, interest, and habituation." For a just evaluation of intelligence tests, the following conditions are of the utmost importance:

1. The social status of the persons examined and their occupational alignment. Differences in these will result in differences in performing the tests.

2. The age data and variations in age or grade in school should be known precisely.

3. Any language handicap must be examined. As Boas (26) points out: "In all tests based on language, the effect of the linguistic experience of the subject plays an important part." "Comparison of reactions of individuals that speak fundamentally distinct languages, may therefore express the influence of language upon the current of thought, not any innate difference in the form of thought." Every group it must be remembered has had an historical and social background peculiar to itself and consequently possesses different modes and patterns of thought. We have shown above the effect of language handicap on a number of tests.

4. How many of those examined are foreigners, and the duration of time they have lived in the new country should be known.

5. The variability in the group examined and the amount of overlapping among the different groups should also be known.

6. More needs to be known about the relationship between physical and mental growth. Dearborn (7), as we saw above, thinks that the superior intelligence of the Jewish child is due to his more rapid anatomical and physiological development. Miss Wentworth in 1926 (27) found a definite relationship between mental and anatomical development. In about two-thirds of her cases retardation in anatomical development accompanied mental retardation, and acceleration in anatomical development accompanied mental superiority. It may well be that the superiority of one year in mental age which the children of the upper classes exhibit is simply an indication of their earlier maturity.

Cozens in 1927 (28) in a similar study showed that:

1. The better developed a boy or a girl is for his or her age the more able are they in school work.
2. The general level of the physical development of children who rate high on intelligence tests is superior to that of children who are low on intelligence tests.
3. There seems to be a direct relationship between ability and physical tests and promotion or scholastic results.

Freeman and Carter (29) and Abernethy (30), on the other hand, studying such items as dentition, age of maturity, height, weight, and the ossification ratio of the carpal bones, and correlating these with results on the Stanford-Binet tests, found that children who are precocious in physiological development are not necessarily of high intelligence.

As regards the test itself, it is as yet uncertain whether the I.Q. itself remains constant as the children tested grow older or are transposed to a new environment. Freeman (31) found an increase of from 7 to 10 points in the I.Q. of foster children placed in a good environment, and Burke found an increase of from 3·9 points (32). It is now realized that a single I.Q., whether based upon an individual or a group test, is unreliable. Symonds (33) lists twenty-five factors that can effect the reliability of a test as, for instance:

(*a*) the construction of words in a test;
(*b*) the speed of taking a test;
(*c*) accuracy of a test;
(*d*) the incentive or effort put in;
(*e*) the obtrusion of competing ideas;
(*f*) distraction;
(*g*) accidents occurring during the examination;
(*h*) illness, worry or excitement;
(*i*) the more common the experiences called for in a test are to the members of the group taking the test, the more reliable it is;
(*j*) the more homogeneous the material of a test the greater its reliability;
(*k*) chance in getting a correct answer to an item in the test is a factor in test reliability;
(*l*) the longer the time a test occupies the more reliable it is.

All these and the personality of the examiner, the subjective and objective elements entering in, the variability in memory and speed of the children, their habits of attention and comprehension, the effect of practice and change in physical condition, will affect the results of a test. It is no wonder, therefore, that competent observers decry the value of intelligence tests. Thus H. N. Bond (34) writes: "The consensus of competent scientific thought, contemplating the inability of mental testers to define intelligence, the inadequacy of all attempts to take such factors as education, social status and language into proper consideration, and the deficiencies of testing conditions, finds no proof of racial inferiority or superiority and eliminates the usual methods of determining such standing from the field of scientific usefulness." Prof. H. C. Link (35) says: "Nothing in the technique of intelligence tests as applied so far, warrants any comparison whatsoever between the intelligence of various groups and races." The opinion of Franz Boas (26) is of special importance. "The test itself only shows that a task set to a person can be performed by him more or less satisfactorily. That the result is solely or primarily a result of organically determined intelligence is an assumption that has to be proved. Since all functions are strongly influenced, by environment, it is likely that here also environmental influences may prevail and obscure the structurally determined part of the reaction." "The responses to tests may be based on recognition of sensory impressions, on motor experience such as the result of complex movements, or on the use of acquired knowledge. All of these contain experience." "A critical examination of all studies of this type in which differences between racial groups in regard to mental reactions are demonstrated, leaves us in doubt whether the determining factor is cultural experience or racial descent. For large racial groups acceptable proof of marked mental differences due to organic not social causes has never been given." The present tendency is to pay more attention to the subtle influence of environmental factors. Even among twins where the influence of heredity was supposed to preponderate it is now seen "that their resemblances are never all exclusive and differ in delicate ways, just as their finger prints may be of the same pattern and differ in the finer measurements (27)." Miss Weill (36) shows in her monograph how many are the varying factors that can enter into the determination of the behaviour of the children of the same family, so that each one's behaviour is different. Even in such remote factors as the quality and condition of the distant and immediate ancestors of an individual, not only germ plasm but social heredity and the influence of tradition are involved. The lasting importance of pre-natal and post-natal conditions are being more and more recognized by doctors and educationalists.

Intelligence tests to have more value than they do now must certainly be improved and made applicable not only to children but also to adults. Furthermore, valid methods for measuring all phases of human behaviour must be devised. To learn anything of the psychological make-up even of a child, we need besides a series of intelligence tests an individual study of the child from all angles, physical, social and emotional—for all these will affect the expression of intelligence. "A low I.Q. does not necessarily prophesy failure, nor a high I.Q. success. Wrong habit reactions formed early in childhood, or in the first years of school life, are at the basis of many neurotic symptoms of much that passes for mental deficiency. This is especially notable in poor habits of attention, memory, and learning (27)."

Besides intellectual differences between Jews and non-Jews, temperamental and emotional differences also are supposed to exist. This view seems very plausible, but little experimental work exists to enable us to speak with any certainty on the matter. Kretschmer (37) is of the opinion that the Jews possess specific types of temperament which are correlated with the high frequency among them of tuberculosis, diabetes, heart disease and arthritis; their distinct physical constitution depends, he believes, on a distinct glandular balance which carries with it mental qualities of a particular kind. Nemeck (1), in his study mentioned above, believes that the Jewish Ashkenazi children are livelier and more emotional than non-Jewish children; the Jewish Sephardi children, on the other hand, are placid and oriental. He is of the opinion, too, that Jewish children think verbally and non-Jewish children pictorially, and that the mental processes of the former proceed in a quicker tempo. In an article in the *Journal of Comparative Psychology* in 1923 (38), it is maintained that Jewish women are more disposed to intellectual pursuits than South Italian women, who take to church-going, and French girls who take to theatre-going and a life of pleasure.

Leydesdorff (39), in a survey of the emotional differences between Jews in Holland and non-Jews, using Heyman's and Wiersma's valuations, found that Jews are much more emotional than Gentiles. The following are some of his results:

	Jews	Non-Jews
Emotionally inclined (%)	63·8	52·6
Quickly offended (%)	52·9	43·5
Good natured (%)	38·6	52·6
Of a critical temperament (%)	53·4	38·4
Anxious and suspicious (%)	41·0	30·9

Jews are also less phlegmatic, but more nervous, sentimental and passionate. They are not as musically inclined as non-Jews, but they are more intellectual. They are fond of good eating and drinking.

	Jews	Non-Jews
Viel auf Essen und Trunken halten (%)	55·9	38·8
Regelmässig Alkohol geniessen (%)	1·9	12 5

Hartshorne and May (40) enquired into the comparative deceitfulness of different national groups, and its relationship to intelligence. They found that the Jewish children from a good environment appear more deceptive than is expected from their level of intelligence; when, however, all class-room cheating tests are combined, Jewish children tend to cheat less than any other race. Jewish children who come from broken homes cheat once in every two opportunities.

Emotional and temperamental tests, being as yet in their infancy and unstandardized, cannot have much value. This also applies to the judgments of psychologists and sociologists on the racial characteristics of the Jewish people. They are of too subjective and contradictory a nature to possess scientific value, although they are of interest in revealing popular beliefs and their origin and crystallization in tradition. Crew (41), the biologist, writes: "They are clever and intuitive, have no capacity to form a political state, but flourish as minorities among other peoples. Inclined to abstraction, logic and dialectics, highly musical, eloquent and monotheistic." Schwalbe and Fischer (42) maintain that the Jew is characterized "in geistiger Beziehung starker Wille, grosse Intelligenz, Lebhaftigkeit und Unternehmungslust, gute Fähigkeiten beim wirtschaftlichen Tauschakt, überhaupt grosse wirtschaftliche Begabung. Die genialen Höchstleistungen des Nordeuropäischen Typus werden allerdings nur selten erreicht." Stoltheim (43), an anti-semitic writer, accuses the Jew of being "sharp at baseness, glib of tongue, greedy for money and of a saving disposition, cunning, addicted to dissimulation, averse to bodily labour, sensual, shameless, vain, cowardly and impudent." And McDougall (44), forgetting his scientific calling as a psychologist, fantastically argues that psycho-analysis "which was evolved by a Jew, who studied chiefly Jewish patients" and which appeals very strongly to Jews, "may be approximately true of the Jewish race." Von Wiessen (45) claims that whilst the Germans are instinctive, plain, kind, healthy, phlegmatic, musical and moral, the Jews are characterized by "Beweglichkeit des Geistes, eine wache Interessiertheit, einen raschen Blick für die Situation, geschickte Beweglichkeit und Vielseitigkeit." Leydesdorff (39) writes: "Ferner überwiegt bei den Holländischen Juden, Ehrgeiz, Geldsucht, Flottheit in Geldangelegenheiten, Herrschsucht, Mitleid, Hilfsbereitschaft, philantropische Tätigkeit, Natürlichkeit im Auftreten, Neigung zum Übertreiben, Ausschmücken,...Furchtsamkeit."

It is interesting to notice that some of the above traits, whether true or not, are *ex post facto* deductions of the Jew *par excellence* as an urban dweller. It is hardly remarkable that a nation for so many centuries commercialized should possess some of these attributes, but that they are innately racial is absurd. An historical study of the opinions that have been passed on the Jew would discover a remarkable sameness and continuity to some extent of these judgments, for tradition having once created a typical and by no means favourable Jew, has passed him down the centuries with little change. Prejudice rather than scientific acumen or dispassionate analysis is responsible for the judgments we make upon members of alien groups.

As regards a number of traits there is no agreement as to whether they are or not characteristic of the Jewish people. Contradictory judgments are frequent, and whilst one psychologist writes that the Jew is distinguished by a passion for abstract thought, another maintains precisely the opposite view. Thus we hear from different psychologists that the Jew is musical and non-musical, sensual and ascetic, sentimental and non-sentimental.

What becomes evident after the review above is that work of value as regards the measurement of racial mental characters and of behaviour which are determined through racially inherited nervous mechanisms does not yet exist. The approach to such knowledge through scientific studies of language is also necessary. Yiddish and Hebrew are in this respect uncharted fields.

Furthermore, the high incidence of certain kinds of nervous diseases among Jews is not racially determined nor correlated with any anatomical or physiological peculiarities in their nervous system. Prevalent functional diseases, such as neurasthenia, hysteria and melancholia are urban diseases, common to urban communities, daily consumed by ambition and anxiety. The long Jewish history of exile and persecution is another cause for the frequency of these diseases. "The Ghetto life was not only unwholesome physically, but unwholesome mentally, emotionally and spiritually. Living in constant dread of massacre, exposed to ridicule, degradation and more sinister disasters, the race developed apprehensiveness and acquired a lower threshold for fear stimuli. This was kept up by the drawing in towards an over-intimate family life (46)." Furthermore, it is to be expected that survivors and spectators of pogroms should have their nervous system shattered.

Biological factors, as distinct from environmental factors which we have stressed hitherto, may have been of more importance in determining the number of mental defectives among the Jews. Living in concentrated and exclusive communities, the Jews frequently inbred, and thus latent defects would rise to the surface. Inbreeding and selection are supposed to explain the excessive number of exceptionally capable and gifted Jews (see Jacobs (47) and Bienstock (48)). Gifted strains frequently married, for a studious son-in-law was preferred to a rich one. We do not know, however, the extent of such inbreeding, nor whether the number of eminent Jews is proportionately larger than in other communities. It is also maintained that the ages of persecution were instrumental in weeding out those who were mentally too feeble to withstand the severe struggle, and leaving the most intelligent and subtle to perpetuate their kind. Hankins (49) writes of the Jews that "their adaptability and intense concentration may be largely racial, having been accentuated by inbreeding and selection, which were favoured by persecution and exclusiveness. These traits are not peculiar to Jews only, though they may have them in greater frequency. Their financial acumen is likewise based on biological and social inheritance." On the other hand, it is equally possible to argue that the finest types of men were eliminated in the persecution.

The unscientific assumption of racial superiority or inferiority has hindered the advancement of racial psychology. An exact technique which will eliminate or make allowance for nurture, and which will secure really random samplings, and evaluate the whole historical environment of a group, will have to be devised before we can range the various races in a hierarchy. As it is the little knowledge that we have acquired on the subject of racial psychology makes once and for all impossible the resuscitation of the Nordic myth, which assumed that such a hierarchy based on innate physical and mental differences exists. History shows us that the achievement of the different nations and groups is no indication of differences in biological worth, and the reaction against the view which would explain complex social phenomena in terms of biology is certainly to be welcomed.

Intelligence tests on Jewish children.

Year	Authority	No. of Jewish children examined	Age (years) or grade	Test	Results
1916	Nemeck	555	High school 15–18	School subjects	Jewish children superior to German
1922	Jacobs	-	10	School subjects	Jewish children slightly inferior to German
1920	Murdoch	489	10–13	Pressey	Jewish boys equal to American whites and superior to Italians and Negroes
1922	Pintner	79	—	Stanford-Binet	Average for all Jewish children speaking English or not was I.Q. 95. Equal to American whites and superior to others
1923	Brigham	Adults	—	Alpha & Beta	Average intelligence below all samples except Poland and Italy; Jews very variable
1924	Feingold	862	High school	Army Alpha modified	Jewish children of high school age have same mental rank as American children in freshman year and are few points below in junior year
1925	Dearborn	—	—	—	Jewish children superior to all groups due to more rapid anatomical development
1925	Fukuda	3	—	—	Median I.Q. of Jews 82 lower than all foreign-speaking groups. Much lower than English group
1925	Graham	47	—	Stanford	Median I.Q. of Jewish children 105. Superior to American (99) and Italian (85). Superior both in verbal and non-verbal tests
1925	Pearson	1204	—	Teachers' estimates	Jewish girls inferior to girls in any type of school. Jewish boys inferior to medium and good school but not to poor school
1925	Luckey	Over 200	Majority 5–8	Terman	Jews had largest number of brightest children and least retarded.* American next
1926	Hirsch	—	Grades I–XI (incl.)	Pintner-Cunningham primary	Rank: (1) Polish Jews; (2) English; (3) Russian Jews; (4) Germans; (5) Americans; (6) Lithuanians, etc.
1926	Bere	—	10	Stanford-Binet. Pintner non-language. National. Pintner-Patterson	In verbal tests: (1) Jews; (2) Bohemians; (3) Italians. In performance tests: (1) Bohemians; (2) Italians; (3) Jews
1926	Goodenough	55	—	Goodenough drawing	Median I.Q. for Jewish children 106·3 highest: low variability
1927	Hughes & Davies	1804	8–14	Northumberland standardized	General superiority of Jewish boys and girls in intelligence and attainments at about every age from 8–13 in every type of school
1928	Seago & Koldin	—	—	—	Jews superior to Italians: not due to language handicap
1928	Hayes	93	University students	Academic record	(1) Chinese; (2) Jews; (3) Nordics
1928	Rigg	445	Grades III–VIII (incl.)	National intelligence test; Woody-McCall arithmetic: Thorndike-McCall reading	Median I.Q. 103, slightly lower than native. Median I.Q. arithmetic 106; reading 98: language handicap involved

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A FURTHER NOTE ON EUPHASIA.

By T. H. PEAR.

(*From the Department of Psychology, the University of Manchester.*)

In this *Journal* (1930, xx, (4), 371) I suggested, with reasons, the term 'euphasia' to signify the ability for deliberate, adequate, verbal expression. The proposal has evoked two published comments.

Prof. C. E. Seashore, *Science*, LXXII, No. 1865, p. 318, acknowledges the need in psychology and education for such a term. Giving further reasons for making this distinction, he comments that 'euphasia' is unavoidably associated with terms of the same root in mental pathology, such as 'aphasia' and 'dysphasia.'

I doubt the unavoidability of the association. 'Eugenic' and 'dysgenic,' for example, are related in precisely the same manner that euphasic and dysphasic ought to be. Moreover, though for practical purposes it may be useful to separate mental pathology from psychology, I hold that theoretical psychology becomes sterile in doing so.

Prof. Seashore's kindly interest has caused him to seek for alternative words. He suggests 'euphany' and 'euphanious.' Yet I am advised by Prof. W. B. Anderson that they are unsuitable. "Although the active voice of phaino means 'show,' the middle voice means 'appear,' and it is the latter meaning that is found in the compounds mentioned above. 'Euphany' could only mean 'good appearance.'"

While, therefore, gratefully acknowledging Prof. Seashore's interest I feel compelled to support 'euphasia.'

In this *Journal* (1930, xxi, (2), 203) Dr H. D. Jennings White urges that this new term be not accepted, on the ground that 'verbal skill' covers all that is necessary. In proposing 'euphasia' I cited a previous article which, I hoped, would represent my reasons adequately ("Some Subtler Skills," this *Journal*, 1929, xx, (2), especially pp. 149 ff.). There it was suggested that anticipation, collection and selection should be regarded as the prelude to skill.

'Verbal skill,' skill with words, might thus be likened to skill with other instruments. A man may be simply skilled with instruments selected for him or he may anticipate, collect and select the instruments, before showing skill. Many reciters, actors and lawyers show verbal skill with words prescribed for them; Prof. William James was euphasic.

A term which "'covers' both vocal and scriptal forms of 'verbal expression'" may be useful. Yet in covering, it obscures the notorious differences between these two arts; expressed in the lines:

"He wrote like an angel, and talked like poor Poll."

Verbal skill marks off a limited, and in many quarters over-rated, ability. One popular movement in psychology suffers seriously from its development of low-grade verbal skill without euphasia.

PUBLICATIONS RECENTLY RECEIVED.

A History of Experimental Psychology. By EDWIN G. BORING. New York; London: The Century Co. 1929. Pp. xvi + 699. $4.00.

As the title of this book indicates, the author is not so much concerned with philosophical psychology as with psychology as an experimental science. Hence he begins by tracing its antecedents in general and evolutionary science, in the physiology of the nervous system and special senses, in the personal equation of the astronomers, and in the popular cults of phrenology and mesmerism. The development of philosophical psychology is then dealt with. Descartes' doctrine of innate ideas was combated by the British empiricists; the greatest of these, Locke, was the founder of associationism, which played so large a part in British psychology, culminating with Mills and Bain. Nativism, developed in Germany by Leibnitz and Kant, was modified by Herbart and Lotze to a more scientific attitude which made possible the foundation of experimental psychology in Fechner's formulation of Weber's law. There followed the great days of 'sensationism' or 'content' psychology, during which the experimental work of Helmholtz and Wundt seemed to show that psychology would rapidly become an exact quantitative science. But the opposing 'act' psychologists, Brentano and his followers, stressed the importance of function rather than content. The former is much less open to introspection and experimentation than the latter. And though Kulpe attempted to treat 'act' and 'content' as co-existent psychological material, it was clear that the problems of psychology could not be solved as readily by experimental methods as Wundt supposed.

With the exception of the school of Titchener, which chiefly developed the sensationism of Wundt, the tendency of American psychology, beginning from the empiricism of James, has been all towards functionalism, and the study of individual differences; hence the modern development of behaviourism, and of mental testing and applied psychology. A further revolt against the analytical methods of the sensationists has also been made in Germany by the 'Gestalt' psychologists. In this country, however, empirical philosophical psychology for long ruled the day, and experimental psychology, with the exception of Galton's anthropometric measurement, is of recent growth.

These various currents of psychological thought and experimental method and their mutual influence are traced by the author with admirable clarity, with a single exception. It is difficult to follow any general trend of thought in American psychology, or, indeed, to distinguish the wood for the trees. This may result from the fact that among the enormous bulk of American psychological work there are few outstanding figures, after James and Titchener. But it may be argued that this lack of clear cut tendency is all to the good. Too often such a tendency has arisen from a philosophical bias towards a theory which will cover the whole field of knowledge, often at the expense of suppression or distortion of experimental evidence. The author of this book points out how behaviourism has been weakened by the inclusion of the whole content of the older systematic psychology. Similarly the Gestalt psychologists have attempted to give their theories a generalized physico-chemical basis. It is the merit of this book that it shows how all these conflicting generalized theories have arisen and developed, and reacted upon and partially destroyed each other; hence the present position of psychology is made clear.

Minnesota Mechanical Ability Tests. By D. G. PATERSON, R. M. ELLIOTT, L. D. ANDERSON, H. A. TOOPS and EDNA HEIDBREDE. University of Minnesota Press. 1930. Pp. xxii + 586. $5.00.

This book is the outcome of an "invitation from Dr Robert M. Yerkes, chairman of the National Research Council Committee on Human Migrations to Donald G.

Paterson and Richard M. Elliott...to submit proposals for research in the field of mechanical ability to be carried out at the University of Minnesota under a grant of financial support from the National Research Council." Dr Yerkes himself contributes a most enthusiastic foreword to the book, and beyond doubt his praise is thoroughly justified. Every person who is interested in methods of studying mechanical aptitudes, and particularly every student who wishes to attempt research in this field will find the volume a mine of information about tests, methods and established results. Moreover the information is generally expressed clearly and concisely. The volume ought to be in the library of every experimental psychological department in this country.

The Growth of Ability. By R. O. FILTER and O. C. HELD. Baltimore: Warwick & York. 1930. Pp. vii + 174. $2.28.

This is a somewhat untechnical general discussion of the learning problem. The authors favour a theory of the environmental determination of learning, assuming only such intra-organic causation as is required by a rather extreme Conditioned Reflex view. The first part of the book consists of a general critical discussion of the problem, mainly in terms of the results or views of other people. The second part presents some practical considerations about learning in an interesting manner. The final *resumé* sides with nurture against nature as the chief condition of learning, but the authors are by no means dogmatic. The volume is No. 28 of Educational Psychology Monographs.

The Psychology of the Common Branches. By W. H. PYLE. Baltimore: Warwick & York. 1930. Pp. vii + 381. $2.10.

The common branches are reading, spelling, handwriting and arithmetic, and this book attempts to set forth the fundamental principles involved in the teaching of these subjects. The exposition is uniformly clear, is accompanied by numerous illustrations, and each chapter is followed by a series of questions and exercises on the matter dealt with. Comprehensive bibliographies are given. The book, being in the main addressed to the working school teacher, is untechnical in style, but it is obviously based upon a great amount of careful experiment and on wide reading, and should most usefully fulfil the purpose for which it was written.

The Meaning of Sacrifice. By R. MONEY-KYRLE. London: Hogarth Press, Ltd. 1930. Pp. 273. International Psycho-Analytical Library, No. 16. 18*s.* net.

The volume consists of two parts, the first dealing with the Oedipus Complex and the second with the Meaning of Sacrifice. The author fully accepts psycho-analytic theories save that he considers it possible to achieve a satisfactory explanation of sacrifice without assuming any psychic continuity in the group or race, further than is secured by age overlap and persistence of institution and custom. Fundamentally sacrifice is based on the Oedipus complex and expresses a tendency towards parricide which may, however, be satisfied by a number of substitute objects. The essay is clearly written and carefully documented, and it expounds quite fairly a number of theories of sacrifice which are different from the one adopted. It is a contribution of value to psycho-analytical study.

Possession Demoniacal and Other; among Primitive Races, in Antiquity, the Middle Ages, and Modern Times. By T. K. OESTERREICH. Translated by D. IBBERSON. London: Kegan Paul, Trench, Trubner & Co. 1930. Pp. xi + 400. 21*s.* net.

The book is a mine of information concerning the phenomena of possession in all ages and amongst widely scattered peoples. Oesterreich maintains both that possession is largely a consequence of belief in spirits—often in malevolent spirits,—and also that above everything else in human history it has served to confirm and deepen this belief. With advancing scientific enlightenment the phenomena become very much less common and the beliefs which are their basis and their consequence tend to disappear. More or less sporadic manifestations still appear but are to be considered as phenomena of 'regression.' The social psychologist will find in this book a mass of interesting and well presented data not overloaded with theoretical matter. The whole work displays wide learning, and a sanely balanced judgment.

Conversations with the Other World, Telepathy in This. Edited by ISOBEL GRANT. London: Williams & Norgate. 1930. Pp. 91. 2*s.* 6*d.* net.

These 'Conversations' are records of automatic writing experiments, the actual 'messages' being printed in italics. Unfortunately no information whatever is given as to the conditions of the experiment, and consequently, though the material may interest a good number of people, psychologically the book is of no value.

Twins; Heredity and Environment. By N. D. M. HIRSCH. Cambridge: Harvard University Press. (London: Humphrey Milford.) 1930. Pp. 159. 10*s.* net.

This is an interesting though not very extensive study of the physical and mental characteristics of twins. The literature of the subject is very well handled and such original observations as were made are clearly set forth. It is concluded that while both heredity and environment play important parts in determining human reaction, the significance of heredity is probably far greater in regard to mental than in regard to physical characteristics. This was particularly marked in relation to I.Q. differences.

The Mind of the School Child. By VALENTINE DAVIS. London: Cartwright & Rattray, Ltd. 1930. Pp. 343. 7*s.* 6*d.* net.

Mr Davis is, he says, "a teacher writing for teachers," and his book is to be judged by its contribution to the practice of teaching. Undoubtedly he has read widely, and has great enthusiasm for his work. Moreover the book contains a large number of lively stories, interesting suggestions and attractive exercises. But it is not well ordered and could reasonably have been considerably condensed. It would have added to the practical value of the work if some indication could have been given of sections that might well be omitted on a first reading. For example the first chapter, bravely entitled "What is Mind?" will probably merely confuse and 'put off' the average teacher. If the volume could be pruned, rearranged somewhat and made a lot simpler in places, it would be greatly improved. As it is it may still be of much help to the school teacher if it is read with discrimination and in a suitably critical frame of mind.

The New Generation; The Intimate Problems of Modern Parents and Children. Edited by V. F. CALVERTON and S. D. SCHMALHAUSEN. With an Introduction by BERTRAND RUSSELL. London: George Allen & Unwin, Ltd. 1930. Pp. 717. 20*s.* net.

This is a co-operative effort produced by thirty-two writers, of whom the majority are American. It is described as: "A handbook for parents and a Magna Charta for children." With one or two exceptions it is distinguished by the unnecessary length with which its authors explain their obvious disagreement with existing institutions and customs. Among the whole of the contributions one essay stands out as a brilliant, original and masterly production: that by Malinowski on "Parenthood—the Basis of Social Structure." This is worth more than everything else in the book put together. Part IV of the volume, which deals with "The Potentialities of the Child," being a little more positive than the rest, contains some interesting contributions, and the paper by Havelock Ellis on "Perversion in Childhood and Adolescence," though it contains little or nothing that is new, is a valuable study. It is a great pity that Malinowski's essay cannot be taken out of this setting and printed separately. Most of the rest could be left without regret to whatever fate awaits it.

Gestalt Psychology and Meaning. By W. D. Ellis. Berkeley: The Sather Gate Book Shop. 1930. Pp. xi + 172.

In spite of a certain—perhaps inevitable—obscurity of terminology, and a certain jumpiness of style which seems odd in an adherent of the psychology known as 'Gestalt,' this is a very good essay. The author is thoroughly conversant with the work of the school whose views primarily concern him, and has also read widely in general psychology. Moreover he is distinctly less intolerant in his attitude to more analytic methods than many recent writers have been. Whether he clears up the intractable problems of meaning or not he certainly gives a fair and full account of many interesting experiments. The first part of the essay is general and considers Elementaristic Psychology, Teleological Psychology, and 'Gestalt' Psychology. The second part deals fairly concretely with specific problems of meaning. The third part discusses and abundantly illustrates 'Gestalt' interpretations of certain phenomenological data, and the fourth part chiefly discusses verbal and mental behaviour and the relation of meaning to other problems. The Lexicon which forms Appendix A will be particularly welcome to many students. Appendix B concerns the Philosophy of 'Gestalten.' The whole essay is rather speculative in bias but is, nevertheless, a very able piece of work.

Ethical Problems; An Introduction to Ethics for Hospital Nurses and Social Workers. By Beatrice Edgell. London: Methuen & Co. 1929. Pp. ix + 149. 5*s.* net.

Although this book is addressed specifically to hospital nurses and social workers it could be read and studied with great advantage to themselves by all students, within college or without, who are approaching the study of ethics. The main reason for this is that Prof. Edgell arrives at her ethical problems by a study of the basic character and determinants of human action. Consequently her discussions have an air of reality which is in welcome contrast to the word-spinning of many widely used ethical text-books. In Part I, which concerns 'principles,' several of the traditional ethical problems are considered, always with a most careful definition of the terms used. Part II concerns 'practical applications,' and is full of acute psychological observation. Although the book is very conspicuously clear both in style and in matter, it is not popular in the sense that it can be read lightly and thoughtlessly. It does indeed excellently fulfil its purpose.

Introduction to Mental Hygiene. By E. R. Groves and Phyllis Blanchard. London: Gerald Howe, Ltd. Pp. vi + 467. 16*s.* net.

This is a general text-book dealing with the healthy regulation of life from childhood, through adolescence to the adult stage in many different spheres. Each chapter contains a discussion, in somewhat general terms, of some special problem, such as Mental Disease, Mental Hygiene and Marriage, Mental Hygiene and Business, and is followed by suggestions for class-room discussions and for written work. A selected Bibliography, as a rule rather too full, is added to each discussion. The writing is uniformly clear, generally very sensible and is well informed. As an introductory work the book may be pronounced a success.

The Measurement of Man. By J. A. Harris, C. M. Jackson, D. G. Paterson, and Richard E. Scammon. University of Minnesota Press. 1930. Pp. vii + 215. $2.50.

J. A. Harris writes on "The Measurement of Man in the Mass"; C. M. Jackson on "Normal and Abnormal Human Types"; D. G. Paterson on "Personality and Physique"; R. E. Scammon on "The Measurement of the Body in Childhood." The scope of the book is accurately indicated by these titles. Each essay gives a mass of carefully collected and sifted information and succeeds in presenting the data both clearly and in an interesting manner. Moreover there is incorporated a considerable amount of original observation. The whole book is a valuable contribution to human science, and deserves a cordial welcome.

Academic Prognosis in the University. By H. A. Egerton. Baltimore: Warwick & York. 1920. Pp. vii + 83. $1.88.

This volume is No. 27 in Educational Psychology Monographs. The author explains a scheme of progressive prognosis to be applied to University students, beginning with Intelligence Tests on entry and continuing by the use of combined results of intelligence tests and achievement performances throughout a student's University career. Considerable and increasing accuracy of prediction is indicated, and it is claimed also that the method may have considerable bearing upon personality assessments.

Educational Achievement in Relation to Intelligence. By C. W. St John. Harvard Studies in Education, vol. 15. Cambridge: Harvard University Press. 1930. (London: Humphrey Milford.) Pp. xiv + 219. 16*s.* net.

The general problem is "What is the precise relation between ability as indicated by *general* experience and achievement and school abilities?" An extensive survey reveals a fairly close agreement between the two with, however, considerable discrepancies. These discrepancies are closely studied and an attempt is made to trace their causation and significance. The whole work is very thorough and issues in many interesting suggestions both of theoretical and of practical import. Eight pages of classified bibliography follow the argument of this clearly designed and carefully executed piece of research.

The Modern Parent. By G. C. Myers. London: Williams & Norgate, Ltd. 1930. Pp. xiii + 350. 8*s.* 6*d.* net.

If there is safety in a multitude of counsellors the modern parent should be in a very happy position. At any rate, whatever he does he is now fairly certain to be able to shelter himself behind somebody or other who has written in the name of psychology to say that this particular procedure is a sound one. These remarks are not intended to cast any unpleasant aspersions upon the book under notice, which is in fact a very clear, sane and level-headed one. The parent may be horrified to learn what a very large number of unfortunate predicaments he may ignorantly land his children into, but at least some excellent advice is given him by following which he may reasonably hope to avoid the worst. Often somewhat vague, sometimes sententious, occasionally ponderous, the book certainly manages to avoid undue technicality. If any criticism is called for, the most obvious point to make is that the author is more emphatic and expansive on what must not be done than upon what should positively be attempted.

Religion and the Reign of Science. By F. L. Cross. London: Longmans, Green & Co. 1930. Pp. 110. 4*s.* net.

One chapter in this book deals with Psychology and Religion. This chapter is not very well-informed and is very limited in its outlook. The author's bias is towards philosophy, and the only psychological work he considers even in scanty detail is that of the behaviourists, that of the psycho-analysts, and some of the work of people interested in psychical research. Concerning this nothing new is said. However, the rest of the book which deals with other departments of science and knowledge may be upon a higher plane.

Guiding the Child. By Alfred Adler and his Associates. Translated from the German by B. Ginzburg. London: George Allen & Unwin, Ltd. 1930. Pp. viii + 268. 10*s.* 6*d.* net.

The bulk of this book is the work of the 'Associates.' Adler himself contributes one short essay on "A Case from Guidance Practice." Most of the discussions are based, either directly or indirectly, upon case history studies in the Child Guidance Clinics of Vienna. A considerable amount of useful illustrative material is presented. In general the discussions are untechnical, intended for the general reader, and rather brief.

Mind at the Crossways. By C. LLOYD MORGAN. London: Williams & Norgate. 1929. Pp. xi + 275. 10*s.* 6*d.* net.

This book is concerned in the main with an interpretation of nature and not with the determination of human reactions or with a study of their genetic significance in human development. Consequently its bias is philosophical throughout, and though many psychological problems are discussed, they are considered mainly in reference to the bearing which they are said to have upon doctrines of 'emergent evolution.' Mind is said to 'stand' somewhere "at the emergent passage from percipience to perception," or, again, "at the emergent switchpoint of divergence from the ascending line of perception to a new and higher line of ascent which comes on the scene with the advent of reflection." It is also thought that there may be Mind at every new departure of natural development, though not within every such departure, and that *this* mind may be called 'Divine Purpose.' Taking it altogether the book is not as concise as it might be, or as clear; and though, as always, Prof. Lloyd Morgan has many striking and attractive things to say, he lets his bent for simile and metaphor have somewhat too free a fling. A psychologist will often find himself labouring far behind the course of argument whatever he may feel inclined to think of the conclusions.

Our Knowledge of Other Minds. By W. W. SPENCER. New Haven: Yale University Press. 1930. (London: Humphrey Milford.) Pp. 145. 9*s.* net.

There are three kinds of knowledge: direct experience; inference or induction from particular facts; and knowledge based upon practical adaptations taking place in a social community. Our knowledge of other minds is of the third kind, but need not lack assurance on that account. The position is ably argued, the author's main bent being philosophical.

The Revolt against Dualism. By ARTHUR O. LOVEJOY. London: Grant Allen & Unwin, Ltd. 1930. Pp. xii + 325. 15*s.* net.

Spirit. By T. A. BOWHAY. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. viii + 224. 5*s.* net.

Songs of the Soul. By AURA MAY HOLLEN. Hollywood, California: Keat's Publications. 1930. Pp. 95.

A Philosophy of Reality. By E. L. YOUNG. Manchester University Press. 1930. Pp. xi + 266. 8*s.* 6*d.* net.

COMMITTEE FOR RESEARCH IN EDUCATION.

With a view to facilitating the general co-ordination of research in education, the Committee has made its annual enquiry as to researches in progress, or planned, at the various Psychological Laboratories and Training Departments.

The following is a summary of the replies received:-

RESEARCHES IN PROGRESS OR PLANNED, DECEMBER 1930.

BINGLEY TRAINING COLLEGE.

The intelligence and educational abilities of physically defective children.

UNIVERSITY OF BIRMINGHAM.

A statistical enquiry as to the correlation of school and university successes.
Tests of higher mental processes.
Studies in the psychology of early childhood.
Reasons for the choice of occupation among secondary school pupils.
An experimental enquiry into the suitability of the Otis group tests of intelligence for use with matriculated students.
The development of humour in children.
A psychological study of Francis Bacon.
Analysis of examination marks with a view to methods of standardization and adjustments between different training colleges.
Preference of children and adults in poetry and prose literature.
Errors in English composition as revealed in essays written by Higher School Certificate candidates and University post-graduate students.

UNIVERSITY OF DURHAM. ARMSTRONG COLLEGE.

The development of memory in its relation to other mental functions.
An investigation into the variation of perseveration among school children and its effect upon school attainments.

UNIVERSITY OF EDINBURGH. GEORGE COMBE PSYCHOLOGICAL LABORATORY.

Group tests of colour vision for use in schools.
The motivation and accessory concomitants of delinquency, with special regard to adolescents.
The speed factor in intelligence.
Variations in suggestibility with age, sex, and grades of intelligence.
A psychological study of art with special reference to pictorial art.
A psychological investigation of the perception and memory of time relations and the development of time concepts in high school pupils.

UNIVERSITY OF EDINBURGH. Education Department.

A study of "answer-patterns" in tests.
Inquiries into "absolute" scale values for test-items.
Comparison of broadcast, gramophone, silent reading, and oral lesson.
Measurement of the value of oral composition as a teaching method.
Binet tests in two Indian languages.
The improvement of methods and materials in native African schools.
Mathematical enquiries into problems of sampling.
Factors in teaching, learning and testing spelling.
Follow up enquiries of children of known intelligence.

LONDON DAY TRAINING COLLEGE.

The development of aesthetic appreciation in children.
Collection of data for re-standardization of Merrill Palmer performance tests.
Psychological problems in the teaching of geography, with special reference to sex-differences.
Vocational guidance in secondary schools.
Treatment of the dull and backward in elementary schools.
Personality traits and persistence.

UNIVERSITY OF LONDON. Bedford College.

Children's thinking.
Study of the part played by imagery in learning (continuation).
Intelligence in relation to the social environment.
Eyedness and handedness.

UNIVERSITY OF LONDON. King's College.

The perception of tachistoscopically exposed words.
Frustration of will acts and conation.
Perseverance and perseveration.
Influence of affection and will on ergographic performance.
Psychology of evidence. Comparison of testimony in normal and delinquent youths.

UNIVERSITY OF LONDON. University College.

Function of images.
Intelligence of three-year old children.
Comparative value of verbal and perceptual tests.
Ability to reason.
Measurement of the "*g*" of the blind.
Training the imagination.
Tests of musical ability.
Tests of oscillation.
Psychological changes in adolescence.
Oral and written tests of "*g*."
Formal training in arithmetic.
Tests of arithmetical ability.

UNIVERSITY OF MANCHESTER.

Methods of provoking mental imagery.

Psychological problems of radio-drama.

Investigations of auditory eidetic imagery.

The relative economy of distributed and concentrated practice time in learning manual skills.

The effect upon the efficiency of localization of sound of certain factors, including frequency, auditory fatigue and directional adaptation.

NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

The use of psychological tests in the vocational guidance of elementary school children in Fife and Birmingham.

The use of psychological tests in allotting boys to working parties in Borstal Institutions.

The measurement of perseveration and the relation of this quality to introversion, extraversion and leadership.

The nature and measurement of the abilities involved in "intelligent" and "routine" assembling.

The nature and measurement of "social ability."

The devising and standardization of tests for motor drivers.

The devising and standardization of tests for colour-discrimination.

Occupation analysis: analysis of the duties involved and psychological qualities required in professional and higher commercial occupations.

Study of the occupational prospects for boys in Tottenham.

UNIVERSITY COLLEGE, NOTTINGHAM.

The relation between literary expression and personality in children.

Children's comprehension of written language.

The development of language in children from five to eight years of age.

UNIVERSITY OF SHEFFIELD.

The development of reasoning in children.

Studies in children's vocabularies.

An investigation of the influence of immediately antecedent activity on the learning ability of elementary school children between the ages of ten and eleven years.

UNIVERSITY OF SOUTH WALES. CARDIFF.

Variations in the development of normal adolescents.

The rhythm factor in literary appreciation.

The measurement of the intelligence of monolingual and bilingual children.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

December 13, 1930.	"Thinking as an Instinct." By Professor Heidbreder (U.S.A.). "Application of the Theory of Two Factors to Non-verbal Tests." By Dr Stephenson. "Perceptual Tests of 'G'." By Dr Fortes. "Demonstration of some new Tests of Musical Ability." By Mr Drake.

SECTIONAL MEETINGS.

Medical.

November 26, 1930.	"Social Behaviour of the Primates." By Dr S. Zuckerman.
December 17, 1930.	"The History of the Conception of 'Nervous Disorder'." By Dr Millais Culpin.
January 14, 1931.	A paper by Dr Alfred Adler.

Education.

December 1, 1930.	"Some Case Studies of Delinquent Girls Described as Leaders." (From Social Study made in the County of Los Angeles, California.) By Miss S. Clement Brown, M.A.
December 31, 1930.	"Learning how to Study." By Professor T. H. Pear.
February 2, 1931.	"Sublimation: A Correlation between the Experiences of an Educator and Psycho-Analyst." By Miss Ella Freeman Sharpe.

Industrial.

December 18, 1930.	"Incentives." By Mr L. I. Hunt, B.A.

Aesthetics Section.

November 14, 1930.	"An Attempt at Representing the Rhythms of Poetry Accurately." By Miss K. M. Wilson, Ph.D.
December 10, 1930.	"Aesthetics and Philosophy." By Miss L. S. Stebbing.

SCOTTISH BRANCH.

Office-Bearers.

President: Dr R. H. Thouless
Vice-Presidents: Dr R. R. Rusk
Dr E. H. Connell
Secretary: Dr Collins
Committee: Dr Drever
Dr Shepherd Dawson
Mr Kennedy-Fraser
Miss Drummond
Sister Marie Hilda
Mr C. A. Mace

Edinburgh.

November 1, 1930. "Psychology and Social Problems." By Dr Shepherd Dawson.
"Demonstration of Phono-Projectoscope." By Dr Drever.
"'Instinct' as an explanatory psychological concept." By Mr C. A. Mace.

February 7, 1931. "The human factor in production." By J. A. Fraser.
"A new performance test of intelligence." By Mr Alexander.
"The speed factor in intelligent reactions." By J. D. Sutherland.

Glasgow.

December 6, 1930. "The Weber-Fechner Law." By Dr Houston.
"Weber's Law." By Dr R. H. Thouless.
"The analogy between mental images and sparks." By Dr L. F. Richardson.

March 7, 1931. "The development of intelligence in children after Encephalitis Lethargica." By Mr J. C. Conn.
"The survival value of intelligence." By Dr W. R. D. Fairbairn.
"A test of musical appreciation." By Mr Macfarlane.

GERMAN PSYCHOLOGICAL SOCIETY

The XIIth Congress of the German Psychological Society will be held at Hamburg from 12*th*–16*th April* 1931. *Further information may be obtained from Prof. W. Stern, Hamburg.*

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AT 30 SEPTEMBER 1930

Revenue Account for the year ended 30 September 1930

Year to 30 Sept. 1929 £ s. d.	EXPENDITURE	£ s. d.	£ s. d.
50 0 0	*To* Rent		50 0 0
79 6 6	Printing		73 16 10
37 15 9	Hire of Halls		26 13 6
	Office and General Expenses:		
	Secretary	200 0 0	
	Addressing	16 3 6	
	Stationery	8 2 4	
	Postage and Telephone...	34 6 0	
	Travelling Expenses ...	5 10 8	
	Subscriptions to outside Associations	6 16 6	
	Audit Fee 1929	15 15 0	
	Sundry Expenses ...	11 12 1	
300 5 1			298 6 1
262 2 2	Journals Publication Expenses		229 14 0
11 3 0	Teas...		8 10 5
8 0 0	Scottish Branch Expenses		7 18 1
2 12 6	Midland Branch Expenses		5 0
10 0	Bank Charges		8 7
— — —	Committee for Research in Education ...		30 0 0
48 13 11	Library Depreciation		46 2 10
800 9 8			771 18 4
337 15 2	Balance, being Surplus of Income over Expenditure		386 13 3
£1138 4 10			£1158 11 7

INCOME	£ s. d.	£ s. d.	Year to 30 Sept. 1929 £ s. d.
By Subscriptions 1930	1184 10 0		
1920	3 8 0		
	1187 18 0		
Less,			
Resignations and Removals.	129 17 0		
		1058 1 0	1047 0 6
Midland Branch (2 Associate Members) ...		5 0	7 6
Interest on Deposit Account		12 12 9	2 8 10
Interest on Investments		87 12 10	88 8 0
		£1158 11 7	£1138 4 10

30 Sept. 1929 £ s. d.	Liabilities	£ s. d.	£ s. d.
	Sundry Creditors		
	Rent	12 10 0	
	Office and General Expenses	16 4 10	
	Journals Publication Expenses	16 18 6	
	Scottish Branch Expenses	7 18 1	
	Library Expenditure	18 2 9	
22 15 0			71 14 2
50 14 6	*Subscriptions in Advance*		30 17 0
	Publications Reserve		
172 0 0	Balance as per last Balance Sheet	172 0 0	
	Less, Grant made during year	50 0 0	
			122 0 0
	Revenue Account		
	Balance as per last Balance Sheet	3424 0 1	
	Add, Surplus of year	386 13 3	
3424 0 1			3810 13 4
£3669 9 7			£4035 4 6

Assets	£ s. d.	£ s. d.	30 Sept. 1929 £ s. d.
Cash at Barclays Bank Ltd.			
Deposit Account	400 0 0		
Current Account	63 5 5		
		463 5 5	435 5 1
Postage Stamps in Hand		3 4 5	— — —
Investments (at cost)			
£100 New South Wales 5½ °/。 Loan 1924–34	100 0 0		
£1000 4 °/。 War Loan 1929–42	1013 5 6		
£1100 5 °/。 War Loan 1929–47	1117 6 9		
£300 5 °/。 Conversion Loan 1944–64	318 9 9		
	2549 2 0		
Note. Market value of above securities at 30 Sept. 1930 £2560. 13*s.* 9*d.*			
500 National Savings Certificates (dated 20 Feb. 1923)	400 0 0		
		2949 2 0	2630 12 3
Subscriptions in arrear		204 7 0	165 6 6
Library			
Balance as per last Balance Sheet	438 5 9		
Expenditure during year	23 2 9		
	461 8 6		
Less, Depreciation	46 2 10		
		415 5 8	438 5 9
(Of the Expenditure on Library of £150 authorised on 8 March 1928 the sum of £134. 0*s.* 6*d.* has been spent to date leaving a balance of £15. 19*s.* 6*d.* unexpended.)			
		£4035 4 6	£3669 9 7

Note. Stock of Journals. There exists a number of past and present volumes of Journals in Stock, but owing to the possibility that a large proportion of these may never be sold, no valuation can be placed on such stock.

I beg to report that I have examined the foregoing Accounts with the books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shewn by the books of the Society. Investments, which are lodged with the Bank, have been verified by production of Banker's Certificate.

T. BARRETT,
Incorporated Accountant.

63, Eastcote Lane,
Harrow.

12 *November* 1930.

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